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- - -

ERRATA

Page 181, line 12, *for* $\left(\frac{h_2}{h_2}, k\right)$ *read* $\left(\frac{h_2}{h_1}, k\right)$

" " *for* $(h, k_2), (h, k_2)$ *read* $(h, k_1), (h, k_2)\ _{22}\psi\psi_{11}$

" " 13 *for* $\left(h, \frac{k_2}{k_2}\right)$ *read* $\left(h, \frac{k}{k_2}\right)$

" last line, *for* $\left(h_2, \frac{k_2}{k_2}\right), \left(h_2, \frac{k_2}{k}\right)$ *read* $\left(h_1, \frac{k_2}{k_1}\right), \left(h_1, \frac{k_2}{k_1}\right)$

Page 182, line 1, *for* $\left(h, \frac{k_2}{k_2}\right)$ *read* $\left(h, \frac{k_2}{k_1}\right)$

" " 4, *for* $\left(h_2, \frac{k}{k}+dk\right)$ *read* $\left(h_1, \frac{k+dk}{k}\right)$

" " 6, ditto ditto

" " 8, *for* h_2 *read* h_1

" " 10, do do

" " 14, do do

" " 16, *for* $\psi(h_2)$ *read* $\psi(h_1)$

Page 196, line 6 from bottom, *for* "Broadhurst" *read* "Brodhurst"

Page 197, line 10 from top, *for* "occasionally weakens the muscle" *read* "may occasion weakness of the muscle"

Page 324, line 2, *for* "portable" *read* "absolute"

Page 327, line 18, *after* "disc," *insert* "when touching the metal below"

Note to the Obituary Notice of Mr Brunel, p viii, line 16.

The statement that Mr Brunel designed the Bute Docks at Cardiff, although founded on what the writer had reason to consider good authority, has since been ascertained to be erroneous

OBITUARY NOTICES OF DECEASED FELLOWS.

Dr Richard Bright was the third son of Mr Richard Bright, an eminent merchant of Bristol, and commenced his education at a private school in the neighbourhood of that city. He subsequently was placed with the late Dr Carpenter of Exeter In the autumn of 1808, he matriculated at the University of Edinburgh, and attended the general lectures delivered at that celebrated school, but it was not until the following year that he entered upon the studies more immediately connected with the medical profession.

In the summer of 1810 he accepted an invitation to accompany Sir George Stuart Mackenzie and Mr (now Sir Henry) Holland to Iceland, and the departments of Botany and Natural History in 'The Travels in Iceland,' subsequently published by Sir George Mackenzie, were written by Dr Bright On his return from Iceland he studied at Guy's Hospital for two years, and it was here that he acquired a taste for pathological investigations He returned to Edinburgh in 1812, and graduated at that University in the following year. He intended likewise to take a degree at Cambridge, and entered as an undergraduate at Peterhouse, but he kept only two terms, finding the college requirements as to residence incompatible with his professional pursuits He then became a pupil, as he was subsequently a colleague, of Dr. Bateman at the Carey-street Dispensary, and acquired a knowledge of cutaneous diseases under that distinguished disciple of Willan.

When the Continent was opened to travellers by the general peace of 1814, Dr. Bright visited Holland and Belgium, and spent some months at Berlin, where he attended the practice of Horn and of Hufeland, and made the acquaintance of Klaproth, Rudolphi, and Heim, during his residence at the Prussian capital, he acquired a thorough knowledge of the German language. From Berlin he went to Dresden for a short time, and arrived before the close of the year at Vienna, where he attended the practice of Hildebrand, Rust, and Beer, and became acquainted with Baron Jacquin, Prochaska, and the elder Frank. From Vienna he proceeded in the spring of 1815 to Hungary, and, on his way home, passed through Brussels about a fortnight after the battle of Waterloo, availing himself of the occasion to witness the medical and surgical practice among the sick and wounded. His 'Travels in Hungary,' published in the year 1818, had reference to this period, and were deservedly popular His account of the gipsies in that country was especially interesting; and his sketches, from which all the plates in that work are copied, showed his artistic skill to great advantage.

In December 1816 he was admitted a Licentiate of the College of Physicians, and was soon after elected Assistant Physician to the London Fever Hospital. In the zealous discharge of his duties there, he contracted fever during a severe epidemic, and narrowly escaped with his life. In the autumn of 1818 he again set out on a visit to the Continent, and soon after his return to England, in 1820, commenced practice as a physician in London. From this time he concentrated the whole of his public duties on Guy's Hospital, of which he became Assistant Physician, holding that appointment until 1824, when he succeeded to the office of Physician. His devotion to the duties of his profession, and to pathology in particular, throughout the period of his connexion with Guy's Hospital, was most remarkable. During many years he spent at least six hours a day in that great practical school, and his indefatigable industry and unrivalled talent for observation then laid the foundation for his well-known discoveries in renal disease.

After giving lectures on medical botany, Dr. Bright, in 1824, began to lecture on the theory and practice of medicine, and after some years associated Dr. Addison with him in that duty. He also, in conjunction with that gentleman, projected a work intended as a class-book and entitled 'Elements of the Practice of Medicine' Only one volume was published, which appeared in 1839, and is understood to have been chiefly the composition of his coadjutor. Dr. Bright resigned the post of Physician at Guy's Hospital in the year 1843, and on his retirement was complimented by the Governors with the honorary title of Consulting Physician.

In 1832 he was elected a Fellow of the Royal College of Physicians, and in the following year delivered the Gulstonian Lectures, the subject being "The Functions of the Abdominal Viscera, with Observations on the Diagnostic Marks of the Diseases to which the Viscera are subject." In 1837 he delivered the Lumleian Lectures at the College, and he chose for his subject the disorders of the brain. He subsequently presented the College with a descriptive account of the pathological collection of Dr. Matthew Baillie. He became a Fellow of the Royal Society in 1821, and at a subsequent period received the Monthyon Medal from the Institute of France.

Although Dr Bright had occasionally suffered from illness for several years, the final and fatal attack—which was found to have depended on an extensive ossification of the aortic valves—was of short duration He first became seriously ill on the 11th of December, 1858, and on the 15th, at midnight, breathed his last.

Dr Bright's contributions to medical science are both numerous and important His 'Reports of Medical Cases,' contained in two royal quarto volumes, published in 1827 and 1831, embrace investigations on diseases of the kidney, the lungs, and the brain and nervous system, and on the pathology of fever The plates of this great work are all coloured, they were executed under the author's immediate superintendence, and are exquisite samples of the peculiar art of representing faithfully and without exaggeration the morbid appearance of tissues and organs

This celebrated work, rich as it is in the fruits of sagacious and untiring research, by no means comprehends the whole of Dr. Bright's published contributions to medical science The volumes of the 'Medico-Chirurgical Transactions,' from the 14th to the 22nd inclusive (1828—1839), contain various papers from his pen; and to the 'Guy's Hospital Reports,' from their commencement in 1836 up to 1843, the date of Dr. Bright's retirement from the Hospital, he contributed no less than sixteen important memoirs. In the eighth volume there is a paper "On Patients with Albuminous Urine," the joint production of Drs. Bright, Barlow, and Rees, in the preface to which (written by Dr Bright) he thus calls attention to an important innovation in clinical study:—"The few following pages will be found to contain the record of the first attempt which, as far as I know, has yet been made in this country to turn the ample resources of an hospital to the investigation of a particular disease, by bringing the patients labouring under it into one ward properly arranged for observation."

Such were the numerous and varied contributions to general knowledge and to medical science made by Dr. Bright. No physician of the present day has, in our country, surpassed, perhaps none has equalled him, in the extent of his original researches in pathology, and although his fame as a discoverer is founded chiefly on his researches into the pathology of renal disease in connexion with dropsy, his minute and unwearied investigation into the morbid anatomy of the kidney, and his detection of the albuminous state of the urine as pathognomonic of disease of that organ, yet a perusal of his writings abundantly convinces us that he brought the same sagacity and industry to bear upon many other problems in pathology and practical medicine His labours in this country, as those of Andral and Louis in France, of Hufeland, the two Franks, and others in Germany, and Abercrombie in Edinburgh, contributed greatly to establish a more practical mode of investigation than had previously prevailed—a more direct attention to the connexion between the symptoms during life and the post-mortem appearances—a more constant and extensive application of animal chemistry and of the powers of the microscope to the elucidation of morbid conditions. Regardless of systems, a maxim laid down by him in early life, in the preface to his 'Travels in Hungary,' as applicable to the traveller, seems to have guided him throughout his distinguished career "correct observation," says he, "and faithful statement are the cardinal virtues on which his character must depend"

William John Broderip, Esq., was the son of an eminent medical practitioner in the city of Bristol, where he was born on the 21st of November, 1789. Having received a sound training in classical knowledge at a provincial seminary, he proceeded in due time to Oxford, where he entered as a student at Oriel College At Oxford he made the acquaintance of Dr Buckland, who at that time (1809) was Fellow and Tutor of Corpus Christi College, but had as yet scarcely turned his mind to that study in which he afterwards became so distinguished. Young Broderip, on the other hand, carried with him to the University a considerable knowledge of Natural History, and especially of Conchology, with which he had from his earliest years been more or less familiar through means of a collection belonging to his father. Such as it was, the zoological acquirement of the student became of service to the future Professor, who in after-years of well-earned fame, was happy to acknowledge that he got his first practical lesson on fossil shells, and gathered what became the nucleus of his own fossil cabinet, in a walk to Shotover Hill with his young friend Mr Broderip

After taking his degree of B A , Mr Broderip became a diligent student of the law at the Inner Temple He was called to the Bar in 1817, and in 1822 was appointed a police magistrate, in which office he continued till 1856 In the latter period of his career he was elected " Bencher" and " Treasurer " of Gray's Inn, and to him was confided the especial charge of the library of that Society

Amidst his arduous duties as a magistrate, Mr Broderip found time to renew his zoological studies, and to begin the formation of a conchological cabinet, which soon grew into first-rate importance, and became an object of special interest to foreign naturalists visiting London, who seldom failed to profit by Mr Broderip's readiness to open it freely to their inspection This collection was purchased for the British Museum.

Mr. Broderip was elected Fellow of the Linnean Society in 1824, of the Geological Society in 1825, and of the Royal Society in 1828. He was a zealous cooperator in the formation of the Zoological Society, of which he was one of the original Fellows and Members of Council He was Secretary of the Geological Society, conjointly with Sir R Murchison, till the year 1830 His death took place on the 27th of February, 1859, from an attack of serous apoplexy.

These particulars of Mr Broderip's personal history have been abridged from a brief memoir of him by one of his most distinguished friends*, who knew him well, and the following account of Mr. Broderip's scientific labours is borrowed entire from the same source.

"To the 'Transactions of the Geological Society' (2nd series, vol. v. p. 171), Mr. Broderip contributed a paper " On some Fossil Crustacea and Radiata found at Lyme Regis in Dorsetshire " His description of " The Jaw of a Fossil Mammiferous Animal found in the Stonesfield Slate" is published in the third volume of the 'Zoological Journal' To the same periodical Mr Broderip communicated " Observations upon the *Volvox globator*," " On the Manners of a live Toucan exhibited in this country," " On the Utility of preserving Facts relative to the Habits of Animals, with additions to two Memoirs in 'White's Natural History of Selborne,' " " On the mode in which the Boa Constrictor takes its Prey," " On the Habits and Structure of *Paguri* and other Crustacea," a "Notice on the *Mus messorius*," together with several valuable conchological articles. The chief bulk of Mr Broderip's original writings on Malacology was consigned to the 'Proceedings' and 'Transactions' of the Zoological Society I may refer to the Indexes of those collections and publications, and to the 'Bibliographia Zoologiæ et Geologiæ,' published by the Ray Society, for the titles of these numerous and valuable memoirs

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* Prof. Owen in the Obituary Notices of the Linnean Society, 1859.

"Few naturalists have more closely observed—none perhaps have more graphically and pleasingly described—the habits of animals. Mr Broderip's 'Account of the Manners of a tame Beaver,' one of the pets that tenanted his chambers, published in the work entitled 'The Gardens and Menagerie of the Zoological Society' (vol. i, p 167), affords a favourable example of his tact as an observer and power as a writer Had circumstances permitted, he would have been a Field Naturalist second only to Gilbert White. When his friend Professor Owen became, through Royal favour, the tenant of one of the lodges in Richmond Park, Broderip would spend there much time in close observation of zoological phenomena afforded by the garden and the wooded vicinity of Sheen Gate A note announcing the commencement of nidification in the adjacent rookery, or the arrival of a migratory song-bird, would immediately bring the retired Police Magistrate to Richmond Park. Many references to facts so observed are made in those delightful combinations of profound and quaint learning with direct and close observation of nature which were contributed by Broderip to the 'New Monthly Magazine' and to 'Frazer's Magazine,' and which he afterwards collected and reprinted in the volumes entitled 'Zoological Recreations' (8vo, 1847), and 'Leaves from the Note-book of a Naturalist' (8vo, 1851).

"Mr. Broderip was ever ready to aid a brother Naturalist. His collections, his rare zoological library, his pure classical taste and varied accomplishments, made the assistance he was able to give most valuable. We find it freely acknowledged in the early editions of Sir C. Lyell's 'Principles of Geology,' in the 'British Fishes' of Yarrell, in the 'Silurian System' of Murchison, and the 'Bridgewater Treatise' of Buckland. Broderip communicated a most valuable Table of the Situations and Depths at which recent Genera of

Marine and Estuary Shells have been observed,' to the Appendix of De la Beche's 'Researches in Theoretical Geology,' and, in conjunction with Captain King, 'Descriptions of the Cirripedia, Conchifera, and Mollusca collected during the Voyage of H.M.SS. Adventure and Beagle, 1826–30' (Zoological Journal).

"To the 'Quarterly Review' Mr. Broderip contributed articles on the Zoological Gardens, on the Vine, on the Cetacea and Whale-fisheries, on the Writings of Captain Basil Hall, on the Bridgewater Treatise of Dr. Buckland, &c. But the main bulk of this indefatigable student's zoological writings are contained in the 'Penny Cyclopædia,' viz. from Ast to the end, including the whole of the articles relating to 'Mammals,' 'Birds,' 'Reptiles,' 'Crustacea,' 'Mollusca,' 'Conchifera,' 'Cirrigrada,' 'Pulmograda,' &c., 'Buffon,' 'Brisson,' &c., and 'Zoology'

"His last publication, 'On the Shark,' appeared in the March Number of 'Frazer's Magazine' It was the 'first part' of an article on that subject, and bears all the marks of a mind in full intellectual vigour"

Isambard Kingdom Brunel was the only son of the late Sir Marc Isambard Brunel, whose mechanical genius and originality of conception he largely inherited. Young Brunel was born at Portsmouth in the year 1806, at the period when his father was engaged in the construction of the block-machinery for the Royal Dockyard. He received his general education chiefly at the Collège Henri Quatre at Caen, where at that time the mathematical masters were particularly celebrated, and to his acquirements in that science may be attributed the early successes he achieved, as well as the confidence in his own resources which he displayed throughout his professional career.

On his return to England, he was, for a time, practically engaged in mechanical engineering, at the Works of the late Mr. Bryan Donkin, and at the age of about twenty he joined his father in the construction of the Thames Tunnel, and there attained considerable experience in brickwork, the use of cements, and more especially in meeting and providing for the numerous casualties to which that work was exposed. The practical lessons there learned were invaluable to him, and to his personal gallantry and presence of mind, on more than one occasion, when the river made irruptions into the tunnel, the salvation of the works was due

One of his first great independent designs was that for the proposed suspension bridge across the river Avon, from Durdham Down, Clifton, to the Leigh Woods, and the acceptance of his proposal he owed to the fact that, upon the reference of the competing designs to two distinguished mathematicians, for the verification of the calculations, his alone was pronounced to be mathematically exact. Want of funds alone prevented, at that period, the execution of the design, which, however, there are now some hopes of seeing carried into effect by transplanting to that site the present Hungerford Suspension Bridge, which is itself the work of Mr Brunel.

His introduction to Bristol led to his appointment as Engineer to the Docks of that city, which he materially improved. He had been previously engaged in the construction of the Old North Dock at Sunderland, and subsequently he designed the Bute Docks at Cardiff.

In 1833–34 he was appointed Engineer to the Great Western Railway, and whilst engaged on the surveys, he matured his views of the Broad Gauge, relative to which he sustained one of the hardest fought engineering contests on record This work placed his reputation high among Engineers, and henceforth his mental and physical powers were taxed almost beyond those of any other member of the profession. His attention to all the details of even the smallest works was unremitting, and the Hanwell and Chippenham Viaducts, the Maidenhead, and other masonry bridges, the Box Tunnel, and the iron structures of the Chepstow and Tamar bridges on the extension of the Railway to the South and West, attest the boldness and originality of his conceptions, his taste in designing, and his skill in the use of various constructive materials

The partial failure at the opening of the Great Western Railway appeared only to incite his inventive faculties, and to afford a field for the exhibition of his great powers. All the physical impediments were met and conquered, and his perseverance was ultimately crowned with success, in attaining a speed of travelling, combined with comfort and security, hitherto unrivalled.

In the attempted adaptation of the atmospheric system of propulsion to the South Devon Railway he was, however, signally unfortunate, in spite of all the ingenuity displayed; but this failure had the effect of bringing into view a most pleasing feature of his character, for while he duly paid up all the calls upon the stake he had in the undertaking, he at the same time refused to accept the professional emolument to which he was entitled.

His services were in constant demand, in railway contests, before Committees of both Houses of Parliament, and he was employed to construct the Tuscan portion of the Sardinian railways, as well as to advise upon the Victorian lines in Australia, and the Eastern of Bengal.

Intimately, however, as the name of Isambard Brunel will ever be connected with the railway epoch in Great Britain, it is probably as the originator of the system of extension of the dimensions of steam-vessels that he will be best known to posterity

The 'Great Western' steam-ship was his first innovation on the usual system. In constructing that vessel, which was much larger than any previously built, he had the able assistance of Mr Paterson, of Bristol, as the shipwright, and of Mr Joshua Field as the constructor of the engines, and in spite of adverse anticipations, even among practical men, the most triumphant success crowned his efforts, and demonstrated the correctness of his views

His attention was at that time directed to the subject of propulsion by the screw, a subject on which Mr. F P Smith had been long and perseveringly labouring, and the experiments made by Mr. Brunel, in his voyages on board the 'Archimedes,' convinced him of the practicability of the adaptation of the system to large vessels. He then designed the 'Great Britain,' an iron ship of dimensions far exceeding those of any vessel of its period · that the first essays were not entirely successful must be attributed to the fact of the machinery not having been designed by those whose peculiar study it had been to produce engines of the class required for such vessels. The disaster in Dundrum Bay demonstrated the scientific design and the practical strength of the hull of the ship, and the successful voyages since made have proved the correctness of his original views. He was then appointed the consulting Engineer of the Australian Steam Navigation Company, whom he advised to construct vessels of five thousand tons burthen, to run the entire voyage to Australia without stopping. His counsels were, however, not followed.

The 'Great Eastern' was his crowning effort, and to the design and execution of this gigantic vessel, far surpassing in dimensions any ship hitherto constructed, he devoted all his energies. The labour was, however, too great for his physical powers, and he broke down under the wearying task, leaving to Mr Scott Russell and Messrs James Watt and Co., his cooperators in the construction of the hull and engines, the actual completion of the work he had so well and so perseveringly brought up to the day of starting on the trial trip

The disasters attending the launch and the trial trip were perhaps inseparable from so novel an experiment, on so gigantic a scale, but the ultimate results may be looked forward to with confidence. Whatever they may be, the impulse given by Mr Brunel to the construction of large-sized vessels is already felt both in our mercantile marine and in the Royal Navy.

This sketch of the professional labours of Mr Brunel is of necessity brief and incomplete, nor can the details be given of the numerous scientific investigations in which he was engaged, but the devotion, during ten years of considerable portions of his time, to completing the experiments made by his father, to test the application of carbonic acid gas as a motive power in engines, must be mentioned. His special objects of study were mechanical problems connected with railway traction and steam navigation, and although he was not, perhaps, so sound, or so practical a mechanic as his friend, and, at the same time, constant opponent, Robert Stephenson, yet his intuitive skill and ready ingenuity enabled him to arrive at satisfactory solutions. The characteristic feature of his works was their size, and his besetting fault was a seeking for novelty, where the adoption of a well-known model would have sufficed. This defect has been unfairly magnified, wherever the pecuniary results of an undertaking have not reached the preconceived standard; and due allowance has not been made for the difficulties encountered in the prosecution of a new and bold enterprise. It might, perhaps, have been as well if a uniform gauge had been originally established for the United Kingdom,—and such may still be the ultimate result; but we must still admire the indomitable energy and consummate skill with which Mr. Brunel and his coadjutor, Mr. Saunders, pushed the broad gauge and its tributaries westward to Bristol, Gloucester, and through Wales, to Milford Haven,—then south-west to Exeter and Plymouth, and onwards to the Land's End; and after invading the north-west manufacturing district of Birmingham, finally arriving at the shore of the Mersey opposite to Liverpool This alone would have sufficed for the lifetime of many men, and in truth the stupendous labours undertaken by Brunel could scarcely be performed without overtaxing the mental and physical faculties, and eventually causing them to break down

Mr. Brunel was fervently attached to scientific inquiry, he was a good mathematician, and possessed great readiness in the practical application of formulæ He was elected at an unusually early age a Fellow of the Royal Society, and was an old Member of the Institution of Civil Engineers, of whose Council he was one of the Vice-Presidents, he belonged to most of the principal Scientific Societies of the Metropolis, and several Foreign Societies, and was a Knight of the Legion of Honour. A liberal patron as well as a discriminating judge of art, he was himself devoted to artistic pursuits, and his early drawings, as well as his professional sketches, attest his feeling for purity of design.

Of his private character, those only who were admitted to his intimacy could judge correctly Brunel was not a demonstrative man, but there was a fund of kindness and goodness within, which only required to be aroused to stand forth in high relief It has been well said of him by an old friend,—"In youth a more joyous, kind-hearted companion never existed As a man, always overworked, he was ever ready by advice, and not unfrequently, to a large extent, by his purse, to aid either professional or private friends His habitual caution and reserve made many think him cold and worldly, but by those only who saw merely his exterior could such an opinion be entertained. His carelessness of contemporary public opinion, and his self-reliance, founded on his known character and his actual works, were carried to a fault. He was never known to court applause. Bold and vigorous professionally, he was as modest and retiring in private life." He was cut off—in his fifty-fourth year—just when he had acquired that mature judgment which in such a profession as that of the Civil Engineer can be attained only by long experience, and when the greatest work of his life had reached the very eve of completion.

Edward Bury, Esq., was born at Salford, near Manchester, on the 22nd of October, 1794. He received his early education at a school in Chester From early youth he exhibited, in various ways, a taste for machinery and construction, and eventually he became established as a manufacturer of engines and machinery at Liverpool. For some years following the opening of the Liverpool and Manchester, and the London and Birmingham Railways, Mr Bury supplied numerous locomotive engines for those lines, and he appears to have been very successful in practically applying in the construction of these engines various improvements in steam-machinery which had been recently introduced or suggested. The details of his improvements are described in a paper "On the Locomotive Engine," which he contributed to the 'Transactions of the Institute of Civil Engineers.' He also acquired much consideration on the Continent on account of his steam-machinery, and especially for his improved engines employed in the navigation of the Rhone For some years after the opening of the London and Birmingham Railway, Mr. Bury had the entire management of the locomotive department, and it deserves to be noticed, that, whilst his administrative services were duly recognized by the Directors, his tact, judgment, and conciliatory disposition gained for him, in a most unusual degree, the regard and confidence of those employed under him Mr Bury afterwards undertook a similar charge on the Great Northern Railway some time after it opened, and in the mean time he had been engaged in different important engineering works at home and abroad. He subsequently withdrew from active life, and died in his retirement, on the 25th of November, 1858. The date of his election into the Royal Society is February 1, 1844.

Henry Hallam, Esq., was born at Windsor (A.D. 1777). His father was Canon of Windsor and Dean of Bristol; the latter preferment he resigned during his lifetime Mr Hallam was educated at Eton, and to Eton he felt, and evinced throughout life, strong and grateful attachment. Both his sons were likewise educated there. Classical learning, then almost the exclusive study in that school, found a congenial mind in Mr. Hallam, and to the last he took great delight in its cultivation. Already at Eton he had become a sound and accurate scholar. Some of his verses, printed in the

'Musæ Etonenses,' are among the best in that collection, and show his command of pure and vigorous Latin, some fancy, and more thought than is usual in boyish compositions From Eton he passed to Christ Church, Oxford If his academic career was undistinguished, it was because in his time the University offered hardly any opportunities of distinction But he remained a faithful member of the University At the height of his fame he undertook the office of Examiner in Modern History, and Christ Church did herself credit by enrolling his name (he was already Doctor of Laws) among her honorary students created under the new academic system Soon after he left the University, Mr Hallam commenced the study of the Law He entered himself as a member of the Inner Temple, became a Bencher, and took so much pleasure in the society of his legal friends, that, almost to the close of his life, he availed himself of the privileges and discharged the duties of that dignity Some independent fortune, which was gradually increased, and an office under Government, in the Stamp Department,—an office which he held till the dissolution of the Board,—happily placed him above the necessity of striving for the emoluments of his profession, while those legal studies were an admirable preparation for his future career Had he devoted himself to the practice of the Law, there can be no doubt that, although he may not have had the bold and ready eloquence, the pliancy, quickness, and versatility of a consummate advocate, yet his profound, accurate, and comprehensive learning, his indefatigable industry, his sagacity in penetrating to the depths of an abstruse subject, his grave, calm wisdom, which had so much of the true judicial character, might have led him to the highest honours and rank in the Law. It is well, however, for his country, for the cause of letters, and indeed of Constitutional Law itself, that he left the dignity of the Bench or of the Woolsack to his eminent contemporaries, and became—what no other man of his day could have become—the Historian of Constitutional Government and Law. In that character, and in that of a man of letters, he has acquired fame and influence as extensive as the world wide English language, and indeed throughout the whole of Europe, where his works are generally known by translations Mr. Hallam became, by deliberate choice and predilection, a man of letters in the highest and noblest sense. His dignified mind, and we may add, his independent circumstances, as they had placed him above following the Law, so also raised him above following literature as a profession. He was in the enviable position that, while he sought and obtained the fame, he could disdain the drudgery of authorship; and there was no fear that such a mind would degenerate into indolence, or indulge in the serene voluptuousness of literary leisure He was a man of books, but not of books only; he took great delight in society, in which he mingled freely; and his own house was open not only to many attached friends, and to his legal contemporaries, but to statesmen, men of letters, of art, and of science, and to cultivated foreigners, whom he received with easy hospitality. There were few distinguished men in England, or even in Europe, who were not proud of his acquaintance; with many he lived on terms of the most intimate friendship.

Mr. Hallam became early a Member of the Royal Society. Though not strictly to be called a man of science, yet his active and comprehensive mind was sufficiently grounded in the principles of most of the exact sciences, especially of mathematics, to follow out their progress with intelligent judgment, and to watch their rapid advance with the utmost interest In the proceedings of this, and of other kindred societies, particularly the Antiquarian, as well as in the administration of the British Museum, of which he was an elected Trustee, he took part, and always, from his remarkable range of knowledge and sound practical habits, with great advantage.

But though Mr. Hallam had thus early taken up his position as a man of letters, he did not come forward as an author till of mature age, and then, with a publication which had demanded years of industrious research and of multifarious inquiry. It was the grave and deliberate work of a man conscious of great powers, one also (which is more rare) fully conscious of the responsibility attached to such powers, and who well knew that the best faculties and attainments may be wasted, as to permanent usefulness and enduring fame, by that hasty ambition which grasps too eagerly after popular applause, and wearies the public mind by incessant demands on its attention. Till this time Mr Hallam was only known by his general reputation as a well-read and accomplished scholar, and by some articles in the 'Edinburgh Review.' The conductor of that journal, then at its height of power and fame (as appears from recent publications), fully appreciated the value of his aid, the extent and the variety of his attainments, one of his articles on Scott's 'Dryden' was remarkable as blending the courtesy due to a man like Walter Scott with free and independent judgment of his opinions, and at the same time as giving a just but discriminate criticism on the most unequal of our great poets.

It was not till past his fortieth year (A.D. 1818) that Mr. Hallam announced himself to the world as an author; but his 'View of Europe during the Middle Ages' placed him at once in the highest rank of historic writers. Of the great qualifications of a historian, except that of flowing, rapid, living narrative (precluded by the form of his work, which unavoidably took that of historical disquisition), none appeared to be wanting. There was profound research into original sources of knowledge, where they existed; the judicious choice of secondary authorities, which always met with generous and grateful appreciation; sagacity in tracing the course of events and the motives of men; thorough independence of judgment, which cared not what idols it threw down in the pursuit of truth, singular firmness with unaffected candour, above all, an honesty of purpose, which almost resembled a passion (the only passion which he betrayed), a style manly, clear, vigorous,—if inartificial, sometimes unharmonious, yet sound idiomatic English,—an apt vehicle for the English good sense which was the characteristic of the whole There was no brilliant paradox, no ingenious theory to which all the facts must be warped; all was sober, solid, veracious The 'View' was received not only with respect, and with the acclamation of all qualified to judge of such a work, but even with popularity, considering its subject and extent, surprising. It was emphatically described by a high authority of the day as a book which every scholar should read, and every statesman study. Like all great works of the kind, it created and supplied a want in the public mind. The History of the Middle Ages up to this time was a wilderness, which few were disposed or able to penetrate. There had been much laborious investigation, much ingenious speculation on parts of the subject; but it was a labyrinth which wanted a clue,—darkness which repelled, confusion which bewildered The 'View' was as remarkable for its completeness and comprehensiveness as for its depth and accuracy. Though the subjects on which Mr. Hallam dwelt at greatest extent, and it seemed with greatest predilection (as, indeed, of the most importance), were the rise, growth, and development of the governments, laws, civil, political and religious institutions of the European family of nations, yet the book likewise entered with great though proportionate fulness on the progress of customs, inventions, language, letters, poetry, arts and sciences It was enlivened by many passages of fine criticism, the note on Dante, for instance, may be read with high interest, after all that has been subsequently written on the great Italian poet. Since the publication of Mr. Hallam's work, awakened curiosity, the study of the philosophy of history, chiefly by Continental writers, and, above all, religious zeal, have investigated almost every point relating to the Middle Ages with emulous ardour and industry, yet Mr Hallam's work has stood the test, and still maintains its ground Mr Hallam himself, with the modesty inseparable from true wisdom, and only anxious for the promulgation of sound truth, instead of narrow jealousy of trespassers upon his province, watched with careful interest every advance in knowledge on those subjects which he had treated almost without a guide. In a supplemental volume, afterwards incorporated with the original work, he collected from every quarter of Europe whatever in his judgment might throw a broader and clearer light on these dark places of mediæval history.

Nearly ten years elapsed before the publication of Mr. Hallam's second great work, 'The Constitutional History of England,' in July 1827. This was in some respects a continuation of part of the former book, which, among the other polities of Europe, had traced the growth and expansion of the British Constitution during the Middle Ages. It may be almost enough to say of this work, that by common consent it has become the standard authority on its all-important subject. It is constantly appealed to in the Houses of Parliament; it is the text-book in the Universities as well as in the higher schools, and this, from a general infelt acknowledgment of its truthfulness, which overawes and convinces against their will those to whom its doctrines may at first sight seem unacceptable. Nor was this from a cold, stately assumption of superiority to the great questions which have divided Englishmen in all ages. Throughout the work, in which every event which has stirred the passions of men, every character illustrious for good or for evil in our annals, passes in review, and is summoned to judgment, though Mr. Hallam holds avowedly and without disguise his own strong opinions, those of a calm, conscientious Whig of the old school, still there is an enforced impression that nothing could tempt him to be an unfair partisan, that he seeks, and only seeks—and seeks without fear, without compromise, without awe of great names, without respect for popular idolatry—right and truth, justice and humanity, sound law, tolerant religion. If there has grown up a more general accordance of sentiment and opinion on English Constitutional History, if extreme differences have died away, and, so far as past times are concerned, the old party watchwords have nearly sunk into oblivion, if there has been greater general sympathy for the wise and good, more unanimous reprobation of the base and bad, this may in some degree be attributed to the influence of 'The Constitutional History of England'

After another interval of nearly the same length (in Sept. 1838 and July 1839) appeared the 'Introduction to the Literature of the 15th, 16th, and 17th Centuries.' This view of the intellectual development of the world during the most active and prolific period in the history of the human mind, if with Mr Hallam a work of labour (to most others it had been a work of intense labour), was yet a work of love It was the overflow of a mind full to abundance of the best reading on almost all subjects, a disburthening, as it were, and a relief from the stores of knowledge accumulated during a long life. If it be hardly possible for a single mind to achieve a history of literature during three centuries (the work bore the modest title of 'Introduction to the Literature of Europe'), yet much is gained by the unity of the work, by the proportion, harmony and order in the distribution of its parts, and if one mind was capable of passing a fair judgment on such different productions of human thought and imagination, it was that of Mr. Hallam. How well he had read the best authors may be tested by his criticisms on Shakspeare, on Ariosto, on Cervantes, and on some of our older poets; his power of grappling with more profound and abstruse subjects, by his estimate of Locke; while writers of a more dry and uninviting class, scholars, even grammarians, pass before us, if with less minute investigation, with much more than a dull and barren recension of their names.

Only one other work, a small one, bears the name of Mr. Hallam, and that, though printed for private distribution, having been liberally communicated to his numerous friends, may justify at least a passing allusion. It records a melancholy chapter in an otherwise uneventful life to which men of letters might have looked with respectful envy. It pleased Divine Providence to try this wise and blameless man with almost unexampled domestic affliction. He married an excellent lady, the daughter of Sir Abraham Elton. Of many children, four only, two sons and two daughters, grew up to mature age. The eldest son was one whom such a father (for Mr. Hallam, with not much outward show, was a man of the deepest and most tender affections) could look upon with pride, with love, and with hope allotted to few distinguished men What was the promise of Arthur Hallam may be known from the volume of his 'Remains,' printed by his father, what he was in disposition as well as in mind, from the exquisite 'In Memoriam' of Mr Tennyson The blow which bereft Mr. Hallam of this son was frightfully sudden His eldest daughter and his wife followed the first-born to the grave. One son remained, he too, if of less originally speculative and poetic temperament than the elder, with great acquirements and endowments, was gifted also with a gentleness and tenderness of disposition, singularly fitted to be the consolation, the surviving hope of such a father. He too was carried off with almost equal suddenness One daughter remains, married to Colonel Farnaby Cator, and has children. Bowed but not broken by these sorrows, Mr Hallam preserved his vigorous faculties to the last, and closed his long and honoured life in calm Christian peace.

ARTHUR HENFREY was born at Aberdeen, of English parents, on the 1st of November, 1819. He studied medicine at St. Bartholomew's Hospital, and in 1843 became a Member of the College of Surgeons, but delicate health prevented him from engaging in the practice of his profession. Accordingly, having a taste for botany, and having already attained to great proficiency in that science, he thenceforth devoted himself exclusively to its pursuit, and soon acquired a distinguished position among English Botanists. In 1847 he was appointed Lecturer on Botany at St. George's Hospital, and in 1854 succeeded the late Professor Edward Forbes in the Botanical Chair at King's College, he was also Examiner in Natural History to the Royal Military Academy and the Society of Arts.

Notwithstanding his feeble bodily constitution, Professor Henfrey's labours were incessant. Whilst constantly engaged in original investigations, the results of which he made known in various papers which appeared in the 'Transactions' of the Royal and Linnean Societies, the 'Annals and Magazine of Natural History,' and the 'Journal of the Agricultural Society,' his untiring industry also enabled him to furnish numerous translations and abstracts of foreign memoirs to the Natural History Journals, and to give reviews and critical notices of botanical works in these periodicals, as well as in the 'Quarterly Review' Moreover, he translated several independent works from the French and German languages, and composed some valuable elementary treatises on botanical subjects, of which his 'Elementary Course of Botany,' published in 1857, is the last and most important For three years also he conducted the 'Journal of the Photographic Society,' and since 1858 was one of the most active editors of the new series of the 'Annals of Natural History'

Professor Henfrey was a man of an amiable and gentle nature, which neither the pressure of daily toil nor the trying interruption of ill-health could ever ruffle his death, on the 7th of September, 1859, at the early age of thirty-nine, hastened as it probably was by his unremitting exertions, has been deeply lamented by all who knew him

Thomas Horsfield, M D., was born at Bethlehem, in Pennsylvania, on the 12th of May, 1773, of parents who were Moravians, in which Christian communion he lived and died. He was educated for the medical profession in the University of Pennsylvania, where he took the degree of Doctor of Medicine in 1798. Early in life he went to Java, where he passed sixteen years, actively engaged in the pursuit of Natural History, to which he had devoted himself. During his residence in Java, he thoroughly explored every part of the island, in quest of its natural productions From Java he visited Banca, and gave the fullest and best account which exists of that island. After the restoration of Java to the Dutch in 1816, Dr. Horsfield made a long sojourn in Sumatra, and there continued his favourite studies; but having made the friendship of Sir Stamford Raffles, who is said to have imbibed from Horsfield his well-known love of Natural History, he followed that eminent person to England in 1818, and soon after was made Keeper of the Museum of the East India Company, which charge he held till his death, which took place on the 24th of July, 1859, in the 87th year of his age

Whilst in the East, Dr Horsfield diligently collected the plants of Java and the adjacent islands, and the folio volume afterwards published in this country, under the title of 'Plantæ Javanicæ Rariores,' contains figures of selected species from his collections, with descriptions furnished by his friends Mr J J Bennett and the late Mr Robert Brown During his stay in Java also, he contributed various papers on the Geology and Natural History of the Eastern Islands to the 'Transactions of the Batavian Society,' of which he was a member The same collection also contains a very interesting experimental inquiry, by Dr Horsfield, on the physiological action of the Upas Antiar poison, the juice of a tree which was afterwards figured and described in the 'Plantæ Javanicæ' His writings on Zoology are, chiefly, his 'Zoological Researches in Java,' 4to, 1821, the valuable Catalogues of the several zoological departments of the East India Company's Museum, and numerous papers on zoological subjects contributed to the 'Linnean Transactions,' the 'Zoological Journal,' and the 'Proceedings of the Zoological Society' His latest publication is the 'Catalogue of the Lepidopterous Insects in the East India Museum,' of which only the first volume has appeared; it was compiled by Mr. Moore, his assistant, from Dr Horsfield's materials and manuscripts, and under his direction Dr Horsfield had some years before commenced a Catalogue of these insects, of which only two parts were published (1828–29), and this publication, though incomplete, deserves notice, as containing an elaborate Introduction, with a general arrangement of the Lepidoptera founded on their metamorphoses The importance of the transformations of insects in reference to their classification had indeed become early impressed on Dr Horsfield's mind, and he accordingly spent three seasons during his stay in Java in collecting the larvæ of numerous species of Lepidoptera, watching their development, and making careful descriptions and drawings of their successive changes up to the perfect state

Dr Horsfield was a member of various learned societies at home and abroad. He was elected a Fellow of the Linnean Society in

1820, and of the Royal Society in 1828. He was a man of retired habits, but of amiable character and unblemished integrity.

Manuel John Johnson, the late Radcliffe Observer, expired suddenly on the 28th of February, 1859, in the fifty-fourth year of his age. Cut off as he was in the midst of his invaluable labours and in the full vigour of his high intellectual powers, his death has caused a severe loss to science, which, however deeply deplored by the numerous friends who enjoyed the privilege of his intimate acquaintance, will only be appreciated in its full extent when the great and important works which he designed, and so nearly executed, shall have been duly completed.

After passing through the usual course of studies at Addiscombe College, Mr. Johnson commenced his public career in 1821 as an officer in the St. Helena Artillery, and while acting as aide-de-camp to General Walker, then in command of the island, was appointed to the control of a small but efficient observatory, founded by the Honourable East India Company. The establishment came into active service in 1829, and in the short space of three years and a half, a valuable catalogue of 606 stars had been observed by the young astronomer and a single assistant. This catalogue was afterwards published, at the expense of the Court of Directors, in the 'Memoirs of the Royal Astronomical Society,' and obtained for its author the award of the gold medal of that body in the year 1835.

On the termination of this important work, Lieut. Johnson returned to England, and entered the University of Oxford as an undergraduate. On the death of Professor Rigaud, in 1839, he was appointed Radcliffe Observer, and speedily rendered the Observatory one of the most active scientific institutions in the world.

One of his first acts was to obtain permission of the Radcliffe Trustees to publish an annual volume of Observations—thus scorning the life of comparative ease he might have chosen, and giving the world an effectual guarantee for the performance of those self-imposed duties to which he so willingly and faithfully devoted the remainder of his life. He accordingly immediately began the re-observation of 'Groombridge's Circumpolar Catalogue,' adding thereto all conspicuous adjacent stars, and many others of especial interest. To this important work the resources of the Observatory were devoted for more than fourteen years volume after volume has been issued with a regularity hitherto unsurpassed, while the high character for accuracy of the star-places in each annual catalogue has been fixed by the unanimous approval of the most eminent European authorities. To render this great work complete, however, required the reduction of every individual observation to a fixed epoch, and the incorporation of the whole into one general catalogue. Much yet remained to be done, though the work was fast progressing, and had its lamented author been spared another year or two, he would have presented to astronomers a monument of industry and devotion to duty unsurpassed in the annals of British science The point now most to be desired, previous to the publication of the final catalogue, is, a rigorous investigation of the errors of division of the Meridian Circle, which has not yet been attempted, owing to the too numerous and pressing pursuits of the unwearied and enterprising Director.

In 1854 a new project was commenced,—'A Catalogue of Remarkable Objects,' comprising all stars of suspected large proper motion, the binary systems, variable and coloured stars, all bright stars down to the third magnitude, and indeed whatever of interest was observable with a four-inch object-glass In the discussion of proper motion Mr Johnson's plan of procedure was that adopted by Madler, viz, instead of deducing the change of place from *two* most widely distant authorities alone, to employ *all* published positions, and thus to deduce the best possible final value; leaving no broken link, no contradictory observation uncorrected, or at least unnoticed, to raise future discussion as to the value derived Such a method is of course the most laborious, but nothing less can be regarded as definitive, and, to secure the best results, neither time nor trouble was spared.

The most eventful epoch in the history of the Radcliffe Observatory and of its late Director remains to be noticed, viz. the establishment of the fine heliometer—a treasure unique in its improvements, unrivalled in its marvellous powers, but almost the labour of a life to develope and turn to the best account. It was erected by its makers, Messrs Repsold of Hamburg, in October 1849, and was forthwith employed in incessant and toilsome research by its skilful manipulator. The Radcliffe Trustees, with their usual liberality, not only provided and equipped this very costly instrument, but, in order to enable Mr. Johnson to devote himself almost exclusively to its use without interrupting the pursuits of which we have already spoken, granted him an additional assistant. After a careful study of the peculiarities of the instrument, in the course of which his acquaintance with German enabled him to derive most important aid from the learned disquisitions of Bessel, Hansen, Wichman, and other astronomers, an elaborate description of it appeared in the preface to vol. xi. of the Radcliffe Observations. The detection and treatment of certain corrections peculiar to the Oxford heliometer afforded a fine example of Mr. Johnson's suggestive genius. Before commencing the more difficult investigations of stellar parallaxes, he passed, step by step, through a patient and judicious course of training, by the measurement of some well-known double stars, of the diameters of the planets, and of the brighter stars in the Pleiades. In 1851 the heliometer was employed in a novel and purely original manner to determine the light-ratios used by different astronomers in their estimations of the magnitudes of the fixed stars. In the two following years his most successful achievements with this instrument were accomplished, viz. the determinations of the parallaxes of the stars 61 Cygni and 1830 Groombridge. His near agreement with the values obtained by Bessel for the former, and by Professor Peters and Otto Struve for the latter object, was most satisfactory, and gave ample evidence of his complete success in these intricate investigations. In 1854 and 1855 the parallaxes of Castor, Arcturus, α Lyræ, and one of the comparison stars previously used in the case of 1830 Groombridge, became the objects of researches the details of which are given in vol. xvi of the Radcliffe Observations.

The same spirit of enterprise and progress which Mr. Johnson evinced in the purely astronomical part of his duties was manifested in a yet more marked degree in his management of the meteorological department. From a single page of ordinary records of the barometer and thermometer, the subject rapidly expanded, under his treatment, to an extent hitherto unprecedented. The introduction of photography as a means of barometric registration by Mr. Ronalds, and its ingenious extension to the wet- and dry-bulb thermometers by Mr. Crookes, as also, at a later period, to the records of the direction and strength of the wind and the depth and time of fall of rain, added such an amount of anxious labour to the already overwhelming requirements of his office, as for the last three years completely precluded him from making further researches with the heliometer. This is the more to be deplored, as there is reason to fear that the monotonous, though cheerfully endured, fatigue inevitable in reducing so novel and accumulative a process to a regular system, accelerated the sad event which it has been our mournful task to record

An earnest labourer himself, Mr Johnson was ever ready to further the scientific labours of those whom he had it in his power to assist, and it was in this spirit that he sanctioned and encouraged his assistant, Mr Pogson, in making independent researches with the Radcliffe Equatorial, after the hour of closure of the official work of the Observatory, whereby that gentleman was enabled to discover four planets and ten new variable stars

The remembrance of his high social qualities, his refined taste, and extensive fund of ready information in literature and the fine arts as well as in science, his frank and agreeable demeanour, his thorough integrity of purpose, and his unostentatious benevolence, will be long cherished by a wide circle of friends, especially in the University, where he was so bright an ornament and so general a favourite

He was President of the Royal Astronomical Society in the years 1857 and 1858, and was elected a Fellow of the Royal Society in 1856.

Lieutenant-Colonel Charles Hamilton Smith was born in Austrian Flanders, on the 26th of December, 1776, of a Protestant family holding a good position in the province, and partly of British descent He was bred to the military profession, and began his career as a volunteer in the British army in the Netherlands, but soon obtained a commission, and in 1797 was transferred to a regiment in the West Indies. After serving for twelve years in that part of the world, he returned to Europe, and joined the Walcheren expedition as Deputy Quartermaster-General In 1813 he was again employed in the Low Countries, and on this occasion he succeeded, with a handful of men, in capturing the fortress of Tholen, whereby a new and better basis of operations was opened up to the British forces in Brabant. After being engaged, in 1816, on a mission to the

United States and Canada, in connexion with the Foreign Office, he, in 1820, retired from active public duty

In the course of his active military life, Colonel Smith found time for varied study History, Archæology, and Zoology were his favourite subjects, and these he continued to cultivate in his retirement The matured results of his labours he gave to the world in various well-known works, of which he was sole or part author For many of these he had early begun to accumulate materials, especially pictorial representations of the objects described, and for this employment his taste and skill as a draughtsman gave him both inducement and facility

Whilst on the staff of the Horse Guards, he wrote the military part of Archdeacon Coxe's 'Life of Marlborough,' which is said to have excited the interest of Napoleon at St Helena as to its authorship, inasmuch as it showed a practical acquaintance with military affairs scarcely to be expected of a churchman He is also the author of the article "War" in the 'Supplement to the Encyclopædia Britannica,' and of the introductory paper on the "Science of War" in the 'Aide Mémoire' of the Royal Engineers

The first publication in which the powers of Colonel Smith's pencil were called into requisition was the 'Costume of the original Inhabitants of the British Islands,' undertaken in connexion with the late Sir Samuel Meyrick. A still greater work, which was in reality hardly less indebted to Colonel Smith, although his name does not appear in the title, is Sir S. Meyrick's 'Critical Inquiry into the History of Ancient Armour' We are informed that most of the illustrations (blazoned in gold and colours) of that celebrated work were copied, with his full concurrence, from Colonel Smith's drawings

Well, however, as Colonel Smith was known as a historical antiquary of no common order, his reputation as a writer on Natural History was perhaps higher The article "Ruminantia" in Griffith's edition of Cuvier's 'Règne Animal' (1835) was written by him, and many of the engravings in that edition were from his drawings At a later period he supplied the volumes on 'Dogs,' 'Horses,' and 'Introduction to Mammalia,' to Sir William Jardine's 'Naturalist's Library', and in connexion with the same series, he published in 1848 his 'Natural History of the Human Species.' He was also the author of the elaborate articles on Natural History, and

Warfare, in the 'Cyclopædia of Scriptural Knowledge,' edited by Dr Kitto

The extraordinary amount of materials collected by Colonel Smith is not, however, to be estimated only by his published works He has left more than twenty volumes of manuscript notes on the most varied subjects In many instances these notes illustrate his remarkable collection of drawings, amounting in number to many thousands The whole of these valuable collections, as well as his personal assistance, were throughout his life placed freely at the disposal of all to whom they could be of service, and it was at all times sufficient for the Colonel to be assured that by his advice and assistance he could further the objects of the literary inquirer or the artist to ensure his active cooperation

Colonel Smith became a Fellow of the Royal Society in 1824, and of the Linnean in 1826 On the formation of the Devon and Cornwall Natural History Society, he was elected President On his retirement from active military service he fixed his residence at Plymouth, where he died on the 21st of September, 1859

Sir George Thomas Staunton, Bart, D C L, was the only child of the late Sir George Leonard Staunton, who is well known to the public as having accompanied Lord Macartney as Secretary of the first Embassy to China, in the year 1792, and as the author of the account of the Embassy which was afterwards published. He is not less known to those who are acquainted with the history of British India, as having, when Lord Macartney was Governor of Madras, concluded the peace with Tippoo Sultan in the year 1784.

Sir George Thomas Staunton was born in May 1781, and died, after a succession of paralytic seizures, in the summer of 1859. He succeeded his father in the baronetcy in the year 1801 After his father's death he was the last male representative of a very ancient English family, the branch of it from which he was descended having been established as landed proprietors in the county of Galway in Ireland since the middle of the 17th century

Sir George Leonard Staunton had some peculiar notions as to education, which he endeavoured to carry out in the training of his son. The son was brought up entirely at home, under his father's eye; and, except on a few very rare occasions, never associated with boys of his own age, but lived entirely in the society of older persons A tutor living in the house instructed him in the Greek and Latin languages He had masters to teach him Mathematics, Botany, and other sciences, and he attended lectures on the various departments of Natural Philosophy Partly, perhaps, from his not being occupied with the pursuits of other boys, but principally from his being naturally endowed with great powers of application and much readiness of apprehension, he made a remarkable progress in all these branches of knowledge, so that when he was not more than fifteen or sixteen years of age he was as much advanced as many even diligent students when they are eight or ten years older

In the year 1792 he accompanied his father to China, under the nominal designation of Page to the Ambassador For some time before the embassy embarked, and during the voyage to China, he had the opportunity of studying the Chinese language under two native Chinese missionaries from the Propaganda College at Naples, and he soon made such proficiency as to be able to speak it with tolerable fluency, and to copy papers written in the Chinese character In this manner he became very useful to the embassy When the embassy was presented at the Chinese Court, the Emperor inquired for the little boy who could speak Chinese, conversed with him for some time, and good-naturedly presented him with an embroidered yellow silk purse for holding areka-nuts, from his own girdle

On leaving China, Sir George L Staunton engaged a Chinese servant to accompany him to England, in order that his son, by constantly communicating with him in Chinese, might keep up and extend his knowledge of the language.

In the year 1799, having received the appointment of Writer in the factory of the East India Company at Canton, young Staunton proceeded a second time to China He remained at Canton, with some occasional visits to Europe, until the year 1817, having for some time before his final return to England filled the office of Chief of the factory. His residence in China afforded him the opportunity of still further advancing himself in a knowledge of the Chinese language by means of native teachers He was the first among the members of the factory who had ever studied the language of the country in which their duties required them to reside, and thus he became very useful by superseding the necessity of employing native interpreters, in whom (principally from the fear which they had of the local authorities) much confidence could not be placed While residing in China, he made several translations from the Chinese, the principal, and that a work of great importance, being the 'Ta Tsingleu-lee,' or Chinese penal code This last was published in the year 1810 Other translations of much interest, though of inferior importance to this, have been published since

In the year 1816 a second embassy was sent to China, the late Lord Amherst, Sir Henry Ellis, and Sir George Staunton being appointed joint Commissioners of Embassy An account of the proceedings of this Embassy has been published by Sir Henry Ellis. Sir George Staunton, however, printed his private journal, and distributed copies of it among his friends

After his return to England, Sir George Staunton purchased a house and landed property in Hampshire, where he afterwards resided during a part of every year For some time he had the honour of representing South Hants in Parliament He afterwards represented Portsmouth, and continued to do so until he resigned the charge a very few years before he died

After his being finally re-established in England, he occupied himself but little with the pursuits of his early life, though it may have been partly his knowledge of botany that led him to lay out an extensive garden, with numerous hothouses and conservatories, full of the rarest trees and plants

Although his life was prolonged until he had entered on his 79th year, he was always of a delicate frame, and not capable of great physical exertion Others observed in him a certain shyness and awkwardness of manner, of which his peculiar education affords an adequate explanation. But with this he on various occasions displayed great moral courage and determination. Many instances of this might be quoted, but one will be sufficient. On the occasion of the second embassy, the Chinese Court refused to receive it unless the ambassadors performed the ceremony of the *Kotou* before the Emperor Lord Amherst and Sir H Ellis wished that they should do so, but Sir George was so satisfied that it would be regarded by the Chinese as an act of humiliation, and something like the homage paid to a feudal lord, that he positively refused his consent. The

Chinese were aware of this, and threatened to dismiss the rest of the embassy, but to detain him as a prisoner. But he declared that this made no alteration in his view of the subject, that, being convinced that he was right, he was quite ready to take his chance of whatever might befall him, rather than swerve from what he regarded as the strict line of his duty

Mr Robert Stephenson, M P, the only son of the late Mr George Stephenson, was born on the 16th of October, 1803, at Willington Quay, near Newcastle-upon-Tyne, where his father had charge of the colliery engine The rudiments of his education were received at the village school at Long Benton, whence he was transferred, at the age of ten years, to the academy of Mr John Bruce, at Newcastle, which he left at the age of sixteen He then received some instruction in mathematics from Mr Riddell, now the Master of the Naval School at Greenwich, and was apprenticed as a coalviewer to Mr Nicholas Wood, with whom he stayed about three years Mr. George Stephenson having by that time raised himself to the position of a consulting mechanical engineer, and appreciating the advantages of that education which it had not been his own good fortune to receive, determined to send his son to the University of Edinburgh, where Robert Stephenson was entered in 1821 During one session, which was all that could be afforded for him, he followed so indefatigably the lectures of the celebrated Professors Leslie, Hope, and Jameson, that he carried off most of the prizes of the year, and feeling the value of the opportunity, he laboured most assiduously, not only to learn everything that was placed before him, but more especially to lay the foundation for future self-instruction

In 1822 he quitted the university to become the apprentice of his father, at the works then first established at Newcastle-on-Tyne for the manufacture of machinery, and whence proceeded the locomotive engines which were destined to produce such a revolution in the internal communication of all countries.

His health having suffered from unremitting study and close application to his duties at the factory, he accepted in 1824 an appointment to investigate and to report upon some silver mines in South America. This occupied him for nearly four years, and upon his report the Columbian Mining Association was formed. Before his departure for America he had assisted his father in the survey of the line of the Stockton and Darlington Railway, which was opened for traffic on the 27th of September, 1825 During the progress of the works, public attention was so much attracted to the subject, that the project was resuscitated for a railway between Liverpool and Manchester, intended chiefly for the conveyance of cotton from the port to the place of manufacture Mr George Stephenson was appointed to survey the line contemplated for the undertaking, and being afterwards appointed engineer for the construction of the work, in the year 1826, and feeling the need of his son's assistance, he summoned him to England Robert Stephenson, on his return home by way of the United States of America and through Canada, fell in with Trevithick, who was also coming home from South America. That steam locomotion should form a constant topic of conversation between two such men was only natural, and Robert Stephenson, who was already acquainted with and had assisted in carrying out the improvements of his father in the Killingworth and other locomotive engines, was well inclined to listen to what were then considered the "visionary schemes" of Trevithick, whose utmost ideas of attainable speed were, however, so soon to be far exceeded

Whilst engaged, after his arrival in England, in assisting his father in the construction of the Liverpool and Manchester Railway, Robert Stephenson at the same time directed his attention to the systems of railway traction, and was successful in the competition for the prize offered by the Directors of the railway for the best locomotive engine. The result of his experiments, added to the joint Essay by himself and Mr. Joseph Locke, in reply to the Report of Messrs. Walker and Rastrick, which recommended fixed engines and rope traction, led to the settled adoption of the locomotive engine, and contributed materially to decide the question of the general introduction of railways in this country. Soon after this he constructed the 'Planet' engine, at the Newcastle factory, which became the type of all the very successful engines that have been since employed. About the same period the United States Government sent three officers of the corps of Topographical Engineers to examine into and report upon steam locomotion on railways as practised in this country. For these gentlemen Robert Stephenson designed and constructed two locomotive engines, embodying a special contrivance adapting them to traverse the sharp curves of the American railways, and in the majority of the American engines that mode of construction has since been followed. Thus it is to Robert Stephenson that are due the types of the locomotive engines used in both hemispheres

The successful result of the Liverpool and Manchester Railway led to the project of a line between London and Birmingham The survey for this line had been entrusted to Robert Stephenson, who removed to London for the purpose of devoting himself to the execution of the works, which had also been committed to him. They were very heavy, and demanded the exercise of the greatest skill and constant personal attention, especially in such works as the Kilsby Tunnel, where the quantity of water met with threatened to stop the proceedings The railway, which was commenced in 1834, was completed in 1838, and such was the reputation acquired by the engineer, that his advice and assistance were henceforth sought in all the important undertakings of the period, either for the construction of the works or in prosecution of the bills before Parliament. Foreign governments also sought his assistance, and for the attention devoted to the scheme for the Belgian railways, both George and Robert Stephenson received from the King of the Belgians, in 1844, the decoration of the Order of Leopold Robert Stephenson received also, in 1848, the Grand Cross of St. Olaf of Norway for similar services. He was thus consulted on the construction of railways in Belgium, Switzerland, Germany, Norway, Denmark, Tuscany, Canada, Egypt, the East Indies, and other countries During the progress of these large systems of lines, he was called upon to design and to execute many very important and some very novel works, of which we can here only mention the Kilsby and numerous other tunnels, large viaducts, such as the High Level Bridge at Newcastle-on-Tyne, the Victoria Bridge at Berwick, and the Conway and Britannia tubular iron Bridges. The latter great innovation in constructive art, which has since been extended to architectural construction with the greatest success, was at first viewed with great distrust, and it required some considerable time to convince the public of the security of such works: subsequently it was as difficult to settle the question as to the real originator of the system. We have here only to record that in 1855 the Council of Presidents and Vice-Presidents of the French Exposition, awarded to Mr. Robert Stephenson the Great

Gold Medal of Honour for the invention and introduction of the system of tubular plate-iron bridges, and First Class Silver Medals to Messrs. William Fairbairn and Eaton Hodgkinson, for their co-operation in the experiments, and also to Mr Edwin Clark, for his aid in the consideration of the designs and in the construction of the bridge itself As a further recognition, the Emperor added the decoration of the Legion of Honour This system of construction has since been extended to the Victoria Bridge across the St. Lawrence River, in Canada, the total length of which is nearly two miles, in twenty five spans That bridge is the greatest example of the system, which has, however, also been employed on a large scale by Mr Robert Stephenson, in the bridges across the Nile, at Benah and at Kaffre Azzayat, on the Egyptian Railway from Alexandria to Suez The limits of this memoir will only permit the further mention of the remarkable constructions on the sea-shore between Conway and Bangor, for the protection of the Chester and Holyhead Railway, and the recent restoration of the iron bridge at Sunderland, which was the last engineering work upon which he was actively engaged.

Mr. Robert Stephenson was considered as the leader in the celebrated discussion, called the "battle of the gauges," for determining whether the narrow or the broad gauge should be the standard for the Kingdom Events have since proved how correct were his views, and notwithstanding the brilliant talents of his friend, but then opponent, the late Mr Brunel, the broad gauge did not spread beyond a certain district It was, moreover, to his strenuous and persistent opposition that was due the rejection of the atmospheric system of traction, attempted to be introduced on the Dalkey, the Croydon, and the South Devon Railways

For some years after he reduced the sphere of his active professional employment, he was engaged in several important public investigations, such as that of the Consulting Commission of the Metropolitan Sewers, and others He made able investigations and reports upon various great undertakings, of which the Liverpool Water-works may be mentioned as an example, and he wrote the article "Iron Bridges" in the 'Encyclopædia Metropolitana.'

The work of his predilection was, however, the management of the Engine Factory at Newcastle-on-Tyne. To that he devoted himself as a labour of love, thinking over improvements and designing innovations, the necessity for which had become apparent in the working of his lines of railway

In the year 1847, Mr Robert Stephenson was elected to represent Whitby in Parliament, and he continued to sit for that borough until his decease. He did not speak much in the House, but when he spoke he always commanded attention, and on such questions as that of the Canal of the Isthmus of Suez, which he is well known to have strenuously opposed, he carried every one with him He was a very useful member of Committees, and had his life been spared, there is no doubt that his services would have been even more frequently required by the Government, who had already learned to appreciate the honesty and truthfulness of his views

He was devotedly attached to scientific investigations, and, as far as his occupations permitted, he was a frequent attendant at the various learned societies of which he was a member He was elected a Fellow of the Royal Society in 1849, and served on the Council He joined the Institution of Civil Engineers in the year 1830, was a member of Council, and filled the office of President from 1856 to 1858. He was also a Member of the Geological, Geographical, Astronomical, and Meteorological Societies, and of the Institute of Mechanical Engineers, as well as of numerous societies in the country. The honorary degree of D C L was conferred upon him by the University of Oxford in 1857, and he had previously received a similar honour from the University of Durham

A most successful professional career, unceasing activity and industry, combined with wisely considered investments, resulted in producing a very large fortune, which he employed during his lifetime most liberally, and from which at his decease, after providing munificently for his numerous relatives, and recollecting all his friends and dependents, he bequeathed upwards of £25,000 to a few charitable institutions and scientific societies,—and this after having given such sums as £3000 at a time to the Literary and Philosophical Society of Newcastle, to relieve it from debt, and to extend its sphere of utility, especially to young men of the working class—an advantage, by which Stephenson himself had profited in early life, and which he had never forgotten

The health of Mr Stephenson had not been good for the last two years; and just before his last journey to Norway, he had complained of want of strength. Whilst in Norway he was very unwell, and exhibited such symptoms of decided liver complaint as induced his speedy return During the voyage, heavy weather was experienced in the North Sea, and he was very sick and ill On his arrival at Lowestoft, he was so weak as to be carried from his yacht to the railway, and from thence to his bed, at his residence in Gloucester Square, where his state grew so rapidly worse as to leave but faint hopes of his recovery The affection of the liver was deeper than had been at first suspected, and was associated with further internal disease, and although his state was for a time alleviated, there was not sufficient strength to struggle against the malady, which terminated his valuable life on the 12th of October, 1859

Thorough uprightness of character was in Mr Stephenson joined with an amiable disposition, and he conciliated the affectionate regard of all with whom he came into immediate relation To those who stood in need of his bounty his hand was ever open, but his beneficence was without display, and he rejoiced in an occasion of doing good unseen His great care was for the children of old friends who had been kind to him in early life, and many young hearts, who owe their present position to his kind solicitude and generosity, will mourn his irreparable loss

Mr. **John Welsh**, Superintendent of the Kew Observatory, was born at Borland, in the Stewartry of Kirkcudbright, on the 27th of September, 1824 His father was George, the eighth son of John Welsh, Esq, of Craigenputtock, a small estate in that district, which had been in the family from an early period. Mr George Welsh, who was extensively engaged in agriculture, died in 1835, and his widow with his two sons settled at Castle Douglas, where the elder—the subject of this notice—continued his preparatory education, and the younger died in 1841, in his 13th year.

In November 1839, Mr John Welsh entered the University of Edinburgh, and studied Mathematics and Natural Philosophy, under Professors Kelland and Forbes, with a view to the profession of a Civil Engineer. He attained the highest prize but one of his year, also prizes in Classics, he also studied Geology in the lecture-room and in the field, under the late Professor Jameson

In December 1842, Sir Thomas Brisbane, Bart, President of the Royal Society of Edinburgh, with the advice of Professor J D Forbes, engaged Mr Welsh as an observer for his Magnetical and Meteorological Observatory at Makerstoun, under Mr John Allan Broun, F R S, the Director, who, in his Report for 1845, says,—"I owe my best thanks to my principal assistant, Mr John Welsh, for the care and assiduity with which he assisted me on all occasions, whether connected with the making or reducing of the observations The more difficult observations for the magnetic dip, and all the determinations of constants, were made by Mr Welsh and myself"

In 1850, the period originally contemplated by Sir Thomas Brisbane for the duration of the Observatory being completed, Mr Welsh was anxious to obtain some other scientific appointment. To aid him in the attainment of this object, Sir Thomas gave him a letter of introduction to Colonel Sykes, F R S, at that time Chairman of the Kew Committee of the British Association, Mr Broun also wrote to Colonel Sykes, expressing his high opinion of Mr Welsh's scientific ability, and accordingly, at the Edinburgh Meeting of the British Association in 1850, the Kew Committee reported that they had engaged Mr Welsh to assist Mr Ronalds, F R.S, who had, ever since the establishment of the Observatory, gratuitously undertaken the office of Superintendent From this period to within a short time of his death, Mr Welsh devoted himself to scientific labour in that establishment, upholding and, year by year, increasing the efficiency of a physical Observatory, which, without any pecuniary aid from Government, has, since its commencement, been entirely supported by annual grants from the British Association, assisted from time to time by donations from the Royal Society

In 1851, shortly after his appointment to the Kew Observatory, Mr Welsh presented to the Association an elaborate Report on the performance of Mr Ronalds's three Magnetographs, and at the same meeting (Ipswich) he described a Sliding Rule for Hygrometrical Calculations, and one for converting the observed readings of the Horizontal- and Vertical-force Magnetometers into variations of magnetic dip and total force; both sliding rules being devised by himself.

In the same year, the Committee, being impressed with the importance of enabling scientific observers at home and abroad to obtain, at a moderate cost, barometers and thermometers of more accurate construction and trustworthy character than those usually sold, directed Mr Welsh to undertake a series of experiments for that object The results of his labours were most satisfactory, and are fully described in a paper printed in the Reports of the Association for 1853 Accordingly, at the present time, standard instruments are supplied to scientific investigators direct from the Observatory, and barometers and thermometers which have been compared with the standards at Kew, and each accompanied with its special table of corrections, are not only supplied to the Government Departments of the Admiralty and the Board of Trade, but can now be obtained from any instrument-maker at greatly reduced prices

In the summer of 1852 Mr Welsh made four ascents in a balloon, for scientific objects A detailed account of these ascents and of the experiments he performed, with a description of the various instruments employed, and a statement of the general results obtained, was communicated to the Royal Society in 1853, and published in the 'Philosophical Transactions' for that year

Sir John Herschel, in his article "Meteorology" in the 8th edition of the 'Encyclopædia Britannica,' makes the following remarks on Mr Welsh's performance ·—"All the observations were conducted with scrupulous precision, and the reductions very scientifically made, these four ascents leaving nothing to desire in point of instrumental appliances and scientific precision in their use"

In 1854, Mr Welsh, at the request of the Kew Committee, undertook a series of experimental investigations on the action of the mercury in marine barometers, known under the term of "pumping." For this purpose, he went, in company with Mr. Adie, to Leith and back in a steamer, and subsequently to the Channel Islands The result of his observations led him to the conclusion that the tube of a marine barometer should, in order to reduce the pumpings, be contracted, so that the mercury will take about twenty minutes to fall from the top of the tube to the height indicating the true pressure, and that by this means the probable error from the cause indicated would not exceed 0 01 of an inch The account of these experiments was published in the Kew Report for 1853.

In 1855, in consequence of its having been represented to the Committee that Her Majesty's Government were anxious that magnetical and meteorological instruments, showing the advanced state of those sciences in this country, should be sent to the Paris Exhibition, Mr Welsh was requested to proceed to Paris with the instruments and to superintend the arrangements An account of the instruments appears in the Report of 1855 of the Kew Committee In the 'Philosophical Transactions' for 1856 there is a paper of Mr Welsh's entitled "An Account of the Construction of a Standard Barometer, and Description of the Apparatus and Processes employed in the Verification of Barometers at the Kew Observatory," and in the 'Proceedings of the Royal Society,' vol vi, a Report of the general process adopted in graduating and comparing the Standard Meteorological Instruments for the Kew Observatory, also a Report of the graduation of Thermometers supplied from the Observatory for the use of the Arctic Searching Expedition under Sir Edward Belcher

In January 1856, a series of monthly determinations of the absolute magnetical force, and of the magnetic dip, was commenced at the Observatory by Mr Welsh, with instruments provided by General Sabine from his department at Woolwich, and in the same year a set of self-recording magnetometers were constructed these were arranged in the basement of the Observatory, and have been in action since January 1858 In the Report of the British Association for 1856, Mr. Welsh described a process for the graduation of boiling-point thermometers intended for the measurement of heights

In 1857 Mr Welsh was elected a Fellow of the Royal Society—a position, to which his qualifications fully entitled him to aspire, but which his natural diffidence would have led him to postpone, had it not been urged on him by those who appreciated his merits. To his personal friends he always spoke with feelings of gratitude of the mode in which his services to science had been thus recognized

Twenty years having elapsed since the execution of the Magnetic Survey of the British Islands, it was determined by the British Association that another survey should be made. With this view, a Committee, consisting of the same five members by whom the former survey was conducted, with the addition of Mr. Welsh, was appointed, and Mr Welsh undertook the Magnetic Survey of the North British division of the United Kingdom In the summer of 1857 he determined the magnetic elements at thirty-one stations, and in the summer of last year he resumed his labours, completing the survey at twenty-four other stations in Scotland and the adjacent islands.

Through the winter of 1857–58, Mr. Welsh had suffered from an affection of the lungs; and on his return from Scotland, in the autumn of 1858, the disease had evidently made rapid progress. Arrangements had been made which would have enabled him to pass the winter in a tropical climate; but acting under the best medical advice that could be procured in the metropolis, he, accompanied by his mother, proceeded to Falmouth. In that place, by the kindness of Mr. R. W. Fox and his family, Mr. Welsh received every attention which they had it in their power to offer. His only regret during his illness appears to have been his inability to complete the works he had undertaken. He died on the 11th of May, 1859, in the 35th year of his age, not less esteemed for his private worth by those who had the pleasure of his acquaintance, than appreciated for his eminent abilities and valuable services by men of science.

Peter Gustav Lejeune Dirichlet was born at Duren, where his father was Commissaire de Poste, on the 13th of February, 1805. After going through the course of instruction followed in the Gymnasium of Cologne, he went to Paris to continue his studies, and in May 1823 he became tutor in the family of General Foy. Here he formed an acquaintance with the most distinguished mathematicians of France. On the recommendation of Fourier, who was the first to appreciate his genius, and aided by Gauss, Von Humboldt procured for him an appointment in Prussia. In November 1827 he obtained the position of Teacher in the University of Breslau, and in the year following was nominated Professor Extraordinary. Being appointed soon after to lecture at the Royal Military School of Berlin, he became Professor Extraordinary in the University of that place. On the 13th of February, 1832, he was elected a Member of the Royal Academy of Sciences of Berlin; on the 6th of May, 1833, Corresponding Member of the Institute of France; in 1833 Corresponding Member of the St. Petersburg Academy; in 1839 Ordinary Professor of the University of Berlin; in 1846 Member of the Göttingen Academy of Sciences. In 1847 a Professorship in the University of Heidelberg was offered to him. In 1854 he was elected Member of the Academies of Stockholm and Munich, and Foreign

Associate of the French Academy, and in 1855 Member of the Belgian Academy, and Foreign Member of the Royal Society

After the death of Gauss he was appointed Professor of the Higher Mathematics in the University of Gottingen, in the spring of 1855, and entered upon the duties of his office in the autumn of the same year He returned in bad health from an excursion in Switzerland in the autumn of 1858, and died at Gottingen on the 5th of May, 1859

By his death the University of Gottingen has lost not only a distinguished teacher and a man of the brightest intellect, but perhaps the only mathematician of the time likely to succeed in completing the unfinished works of Gauss, a task which he had declared himself willing to undertake

His mathematical memoirs, the first of which was presented to the Institute of France in 1825, are too numerous to admit of introducing their titles into this notice They are published in the 'Transactions' of the Berlin Academy from the year 1833 to 1854, in 'Crelle's Journal' from 1828 to 1857, in the 'Monatsberichte' of the Academy for 1852–1855, and in volumes iv, v, ix, xii of 'Liouville's Journal'

The Baron Friedrich Heinrich Alexander von Humboldt, was the second son of Alexander George von Humboldt, descended from a noble Pomeranian family His father was a major in the Prussian army, and had served with distinction as aide-de-camp to the Duke Ferdinand of Brunswick in the seven years' war The distinguished subject of our present brief notice was born at Berlin on the 14th of September, 1769 At the age of ten years he lost his father From 1787 to 1789 he studied, first for some months in the University of Frankfort on the Oder, and afterwards in that of Gottingen During the vacations, he made geological excursions to the Harz, and on the banks of the Rhine, and published the results of his observations under the title 'Ueber die Basalte am Rhein, nebst Untersuchungen uber Syenit und Basanit der Alten' In the spring of 1790 he made a hasty excursion through Holland, England, and France, in the company of George Forster, who sailed with Cook in his second voyage round the world. On his return from this excursion, he passed some months at Hamburg, preparing himself for a post in the Finance department of his native country In June 1791 he went to Freiberg, where he attended the lectures of Werner, and became acquainted with Von Buch and Del Rio During his residence at Freiberg he collected the materials for his work entitled 'Specimen Floræ subterraneæ Fribergensis et aphorismi ex physiologia chemica plantarum' After obtaining an appointment in the Administration of Mines at Berlin, he filled the office of Director-General of the mines of Ansbach and Baireuth from 1792 to 1797 During this time he prepared his work on the irritability of muscular and nervous fibre On the death of his mother in 1796, his desire to travel increased He resigned the post of Director of Mines, and devoted himself to the study of practical astronomy under v Zach. After passing some months at Jena and Vienna, he made an attempt to visit Italy, accompanied by v Buch, for the purpose of examining the volcanos of that country Being forced by the war of which Italy was the scene to relinquish this undertaking, he passed the winter of 1797–1798 in the study of meteorology at Salzburg and at Berchtesgaden. He was invited by Lord Bristol to accompany an exploring party into Upper Egypt, and went to Paris to procure the necessary instruments On his arrival, in May 1798, he was apprised of the failure of Lord Bristol's project in consequence of the departure of the French expedition for Egypt Here he became acquainted with his future fellow-traveller Bonpland He obtained permission from the Directory to accompany Baudin in his voyage round the world The sailing of this expedition having been postponed, he made an attempt to join the French in Egypt Failing, however, in consequence of the non-arrival of a Swedish frigate in which he had been promised a passage to Tunis, he went to Spain, accompanied by Bonpland, where he passed the winter of 1798–1799 Encouraged by the Spanish Minister, Luis de Urquijo, to visit Spanish America, he and Bonpland embarked at Corunna on the 5th of June, 1799 They landed at Santa Cruz on the 19th of June, ascended the Peak and explored the Island of Teneriffe On the 16th of July they reached the Cumana. After having traversed Venezuela, the valleys of the Orinoco and Amazon, the countries of Peru and Mexico, Humboldt embarked on the 7th of March, 1804, for the Havana, where he remained six months. After visiting the United States, he sailed from America on the 9th of June, and landed at Bordeaux on the 3rd of August, 1804

He made Paris his residence from 1805 till 1827, occupying himself with the publication of the results of his travels, in eight separate works, and with various chemical and physical researches He visited Naples with Gay-Lussac and v Buch in 1805 During the residence of Prince William of Prussia in Paris in 1807–1808, Humboldt held a diplomatic appointment In 1814 he accompanied his elder brother Wilhelm v Humboldt, then sent on an embassy to London, and was elected a Foreign Member of the Royal Society in 1815 His 'Mémoire sur les lignes isothermes' appeared in 1817 He was present at the Congress of Aix-la-Chapelle in 1818, and at that of Verona in 1822, and in the same year accompanied the late King of Prussia to Naples

From the year 1827 he made Berlin his home In 1829 he travelled with Ehrenberg and G Rose, under the auspices of the Emperor Nicholas, through Siberia, as far as the frontiers of China The results of this journey, which lasted nine months, are published in his 'Asie Centrale,' and in G Rose's 'Reise nach dem Ural, Altai, und dem Caspischen Meere'

After the French Revolution of 1830, v Humboldt was commissioned by Frederick William III to recognize the accession of Louis Philippe on the part of Prussia About this time he completed his 'Examen critique de la Géographie du nouveau Continent.' In 1841 he accompanied King Frederick William IV to England, and visited Paris for the last time in the winter of 1847–1848 In 1845 he published the first volume of 'Cosmos,' a work which may be regarded as a development upon an extended scale of his 'Ansichten der Natur,' the third edition of which appeared in 1849. The first part of the fourth volume of 'Cosmos' appeared early in 1858, the second part is so far prepared that no obstacles to its completion are anticipated

He enjoyed good health till near the end of his life. In October 1858, he was attacked by an illness from which he never completely recovered On the 21st of April, 1859, in consequence of a cold, he was unable to leave his bed. He retained the use of his faculties till the morning of the 6th of May, when he became speechless, and died at half-past two in the afternoon of the same day.

In 1852 the Royal Society awarded him the Copley Medal for his eminent services in Terrestrial Physics, and he considered this to be the highest honour he had ever received A letter from Dr. Pertz, the Librarian of the Royal Library at Berlin, to Sir Charles Lyell, states, that the Medal was found after his death amongst the objects which he wished to be for ever preserved in the family archives at Tegel. The outer envelope of the box containing the medal bore this inscription in his own handwriting. "Die wichtige berühmte Copleysche Preis-Medaille der königlichen Societat (in meiner Familie aufzubewahren in Tegel) Al Humboldt Sept. 1858." On the exterior envelope he had written: "Das ehrenvollste, das ich besitze, die berühmte Copleysche Ehren-Medaille der königlichen Societat zu London von 1852 in familien Archiv zu Tegel aufzubewahren Al Humboldt 6 Marz, 1859"

Looking forward to the probable appearance of a complete biography *of this illustrious philosopher and traveller at no very distant period*, it is needless at the present time to enter more fully into the details of his life and labours

Karl Ritter was born at Quedlinburg on the 7th of August, 1779. His education was completed in the University of Halle. In 1798 he became tutor in the family of M. Bethmann Hollweg, and travelled with his pupils through a large portion of Europe He then went to Gottingen, in order to avail himself of the library of that place in prosecuting his researches in ancient history. After four years' assiduous labour, he succeeded Schlosser as Professor of History at Frankfort, he was then chosen Professor of Geography in the Military School, and afterwards Professor of History in the University of Berlin

He was a Knight of the Order Pour le Mérite, Member of the Academy of Sciences of Berlin, Foreign Associate of the French Institute, Foreign Member of the Academies of Sciences of Gottingen, Copenhagen, St. Petersburg, and Munich, Honorary Member of the Vienna Academy of Sciences, and of numerous other literary and scientific societies He was elected a Foreign Member of the Royal Society in 1848. He died on the 28th of September, 1859

The most important of Ritter's works is the second edition of the 'Allgemeine vergleichende Geographie,' the first volume of which appeared in 1822, and the twenty-third in 1859, accompanied by an Atlas, on which Etzel, Grimm, Mahlmann and Kiepert have laboured in succession. This work, though still incomplete (of the three parts the author intended to devote to Asia Minor, only two, parts xviii and xix have been published), is considered the most valuable work on Geography in existence Among the other writings of Ritter, the following deserve especially to be mentioned :—' Europa ein geographisch-statistisches Gemalde' (1807), Vorhalle Europaischer Volkergebilde vor Herodot' (1820), 'die Stupas' (1838), 'die Colonisirung von New-Seeland' (1842), 'Blick auf das Nilquell-land' (1844), 'der Jordan, und die Beschiffung des Todten Meeres' (1850), 'Blick auf Palestina' (1852), 'das Kameel' (1852) Many memoirs by Ritter have appeared in the 'Transactions' of the Berlin Academy, and in the 'Journal of Universal Geography' Some of these have been collected and published under the title of 'Einleitung zu einer mehr wissenschaftlichen Behandlung der Erdkunde.'

These labours entitle M Ritter to be considered as the creator of scientific geography Instead of limiting geography to collecting isolated facts, and descriptions without logical order, he tried to discover the natural and intimate relations which exist between a country and its inhabitants employing all the ideas which history and the natural sciences supply, he has drawn conclusions, which have erected the geography of the present time into a kind of physiology of the earth

PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 5, 1859

Sir BENJAMIN C BRODIE, Bart, President, in the Chair.

In accordance with the Statutes, the President read the following list of Candidates recommended by the Council for election into the Society —

Samuel Husbands Beckles, Esq
Frederick Crace Calvert, Esq
Henry J Carter, Esq
Douglas Galton, Esq
William B Herapath, M D
George Murray Humphry, Esq
Thomas Sterry Hunt, Esq
John Denis Macdonald, Esq
William Odling, Esq
Robert Patterson, Esq
John Penn, Esq
Sir Robert Schomburgk
Thomas Watson, M D.
Bennet Woodcroft, Esq.
Lieut -Col. W Yolland

The following communications were read:—

I. "Propositions upon Arithmetical Progressions." By F. Elefanti, Esq. Communicated by Arthur Cayley, Esq. Received April 6, 1859.

(Abstract)

The author sketches the investigation of the way of throwing various series of integer terms into arithmetical progressions, such as the sums of squares, cubes, &c of figurate numbers, of the powers of a number, &c. He also gives the resolution of a given number,

in certain cases, into an arithmetical progression. Thus, having the theorem that N can be resolved into an arithmetical progression when 16 N+1 is a square, he is enabled to detect factors in N, he thus shows that 2079519603 has 43 and 101 among its factors. Among theorems which the method gives, may be noticed the following, as one of a peculiar and unstudied class. If in the series 1, 3, 5, 7, &c. four terms be taken, and the next *one* omitted, then the four next terms taken and the next *three* omitted, then four terms taken and *five* omitted, and so on, the four terms taken will in every case consist of numbers prime to one another.

II. Abstract of a Memoir "On the Electric Properties of Insulating or Non-conducting Bodies." By Professor CARLO MATTEUCCI of Pisa. Communicated by Major-General SABINE, R.A., V.P. and Treas. R.S. Received April 14, 1859.

The object of the author in the first part of this memoir is to ascertain by experiment what condition is assumed by insulating or non-conducting bodies in the presence of an electrified body, and in what degree such condition is developed in insulating bodies of different kinds. In a memoir published nearly ten years ago (Ann. de Chim. et de Phys., xxvii. p. 134), he had shown that a cylinder of gum-lac, sulphur, stearic acid, or the like, suspended by a filament of silk, and brought near to a body charged with electricity, begins to oscillate in the same way as a cylinder of metal. The non-conducting cylinder, whilst under the influence of induction, behaves like any body charged with opposite electricities, and returns to its natural state when the induction ceases.

These experiments have now been very carefully repeated with cylinders formed of various insulating substances, made as nearly as possible of the same length and perfectly diselectrized. The air was rendered perfectly dry, and the inducing ball was charged with electricity to a constant degree, measured by the torsion-balance.

After giving a numerical statement of the time of oscillation and the moment of the induced force, as determined by experiment for cylinders of different insulating substances, and after describing other experiments intended to prove the insulating property of the materials employed, the author goes on to observe that there is but one way of explaining the phenomena in question, namely, by supposing that the individual particles or molecules of the non-conducting cylinder acquire different electrical states at their opposite extremities, and that these electrical states, while they are readily developed and neutralized within each particle, meet with great resistance in passing from one particle to another,—a condition of non-conducting bodies which constitutes the *molecular electric polarization* of Faraday

The author then gives the result of some experiments on the amount of electrical charge communicated to, or given out by an insulated conducting ball surrounded, at one time by air, at another time by an insulating substance, such as sulphur

In conclusion, the author thinks that the following propositions may be regarded as rigorously demonstrated by experiment —

1 The effects produced on insulating cylinders in the presence of an electrified body, depend on the state of *molecular electric polarity* which that body developes in the cylinders, and thus the hypothesis of Faraday is directly demonstrated by experiment.

2 Other circumstances being alike, the insulating power of a substance is greater in proportion as its degree of polarization is weaker

3 The electric capacity of a conducting body—that is, the quantity of electricity which it acquires when placed in communication with a source of electricity—is much greater when the body is surrounded with sulphur, or some other solid isolating substance, than when surrounded by air Similarly, the body being electrified from the same source and then surrounded with sulphur, or else surrounded with air, afterwards yields to the same conductor much less electricity in the former case than in the latter

4 The effects produced by insulating plates interposed between the armatures of a Leyden jar or of a magic square are explained, together with the phenomena previously described, both by the penetration of the electricity into the interior of the insulating substance, and the propagation of electricity along the surface of the plates

III. "On the Synthesis of Acetic Acid" By J A. WANKLYN, Esq. Communicated by Dr E FRANKLAND. Received April 27, 1859

I have elsewhere* shown that a salt of propionic acid results when carbonic acid is brought into contact with a compound consisting of ethyl and an alkali-metal Guided by a well-known principle, I also inferred that an analogous reaction is common to the whole vinic series

Believing, however, that it was desirable to investigate other members of the series, I have since undertaken the case of the corresponding methyl-compound, and find that it fully bears out the law, as will be manifest from the following details

Some sodium-methyl in mixture with zinc-methyl, zinc, sodium, and ether was obtained by acting with sodium upon a strong ethereal solution of zinc-methyl The product so obtained was divided into two portions—one of which was exposed to the action of a current of dry carbonic acid, and the other reserved for comparison

During the transmission of carbonic acid, the sodium-methyl became hot After the completion of the reaction, the resulting solid was treated with a little mercury, in order to convert any free sodium into an amalgam, which would not decompose water with too great violence

Subsequent distillation of the product, with excess of dilute sulphuric acid, yielded a distillate having most distinctly the smell and taste of acetic acid This acid distillate was redistilled, when it proved to be free from sulphuric acid.

Some of it was converted into a silver-salt by digestion with oxide of silver This silver-salt was dissolved in hot water, filtered hot, and allowed to crystallize on cooling An abundant crop of crystals separated, which was drained from the mother-liquor, the employment of a filter being avoided The crystals were afterwards dried *in vacuo* over sulphuric acid until they no longer lost weight.

Determinations of silver were made by ignition, the resulting silver being reheated and reweighed until it remained constant

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* See Quarterly Journal of the Chemical Society, July 1858, page 130

I 0894 gramme of the salt gave 0580 gramme of metallic silver
II 1597 gramme of the salt gave 1022 gramme of metallic silver

Comparison of these results with the composition of acetate of silver gives as follows —

	Calculated	Found	
		I	II
Per-centage of Silver	64 67	64 88	64 00

which leaves no doubt as to the presence of acetic acid

Still further proof of the same was obtained by converting a little of the acid into a soda-salt, and heating it with arsenious acid Abundance of kakodyl was evolved

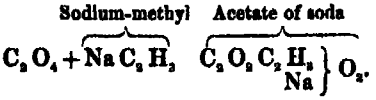
In order to remove any doubt which might exist as to the source of this acetic acid, and to show that it did not arise from oxidation of the ether which accompanied the sodium-methyl, I operated upon some of the original sample which had never been exposed to carbonic acid, and which, as previously mentioned, had been laid aside for comparison

I mixed it with a little mercury, distilled with excess of dilute sulphuric acid, and digested the redistilled distillate with oxide of silver, using the same samples of acid and of oxide of silver as in the former experiment

The distillate neither smelt like acetic acid, nor yielded acetate of silver on spontaneous evaporation to dryness *in vacuo* of its product with oxide of silver Neither could kakodyl be obtained on heating its soda-residue with arsenious acid

From all which it is evident that the acetic acid obtained must have been the product of the action of carbonic acid The following conclusion is, therefore, established —

Dry carbonic acid is decomposed by sodium-methyl with evolution of heat, and production of acetate of soda.



$$C_2O_4+\overbrace{NaC_2H_3}^{\text{Sodium-methyl}}\quad\overbrace{\left.\begin{matrix}C_2O_2C_2H_3\\ Na\end{matrix}\right\}O_2}^{\text{Acetate of soda}}.$$

I hope also to be able to present shortly an account of the action of carbonic acid on one of the higher compounds of the alkali-metals—most probably on potassium or sodium-amyl

May 12, 1859

Sir BENJAMIN C BRODIE, Bart., President, in the Chair

The following communications were read —

I "On the Resistance of Glass Globes and Cylinders to collapse from external pressure, and on the Tensile and Compressive Strength of various kinds of Glass" By WILLIAM FAIRBAIRN, Esq, C.E., F R S, and T. TATE, Esq, F R A S

Received May 3, 1859

(Abstract)

The researches contained in this paper are in continuation of those upon the Resistance of Wrought-Iron Tubes to collapse, which have been published in the 'Philosophical Transactions' for 1858 The results arrived at in those experiments were so important as to suggest further inquiry under the same conditions of rupture with other materials, and glass was selected, not only as differing widely in its physical properties from wrought iron, and hence well fitted to extend our knowledge of the laws of collapse, but because our acquaintance with its strength in the various forms in which it is employed in the arts and in scientific research is very limited. To arrive at satisfactory conclusions, the experiments on this material were extended so as to embrace the direct tenacity, the resistance to compression, and the resistance to bursting, as well as the resistance to collapse

The glass experimented upon was of three kinds:—

	Specific gravity
Best flint-glass	3 0782
Common green glass	2 5284
Extra white crown-glass	2 4504

Tenacity of Glass —For reasons detailed by the authors, the experiments upon the direct tenacity of glass made by tearing specimens asunder are less satisfactory than those in the rest of the paper, and it is argued that more reliance is to be placed upon the tenacity deduced from the experiments on the resistance of globes to bursting in which water-pressure was employed, than upon the tenacity obtained directly by tearing specimens asunder. The

results obtained by the latter method give the following mean results:—

	Tenacity per square inch in pounds
Flint-glass	2413
Green glass	2896
Crown-glass	2346

Resistance of Glass to Crushing —The experiments in this section were made upon small cylinders and cubes of glass crushed between parallel steel surfaces by means of a lever The cylinders were cut of the required length from rods drawn to the required diameter, when molten, and then annealed, in this way retaining the exterior and first cooled skin of glass. The cubes were cut from much larger portions, and were in consequence probably in a less perfect condition as regards annealing Hence, as might have been anticipated, the results upon the two classes of specimens, although consistent in each case, differ widely from one another

The mean compressive resistance of the cylinders, varying in height from 1 to 2 inches, and about 0 75 inch in diameter, is given in the following Table —

Description of glass	Height of cylinder in inches	Mean crushing weight per sq in		Mean crushing weight per sq in	
		in pounds	in tons	in pounds	in tons
Flint-glass	1	29,168	13 021	27,582	12 313
	1·5	20,775	9 274		
	2 0	32,803	14 644		
Green glass	1	22,583	10 081	31,876	14 227
	1 5	35,029	15 628		
	2 0	38,105	16·971		
Crown-glass	1 0	23,181	10 348	31,003	13 840
	1 5	38,825	17·332		

The specimens were crushed almost to powder by the violence of the concussion, it appeared, however, that the fracture occurred in vertical planes, splitting up the specimen in all directions Cracks were noticed to form some time before the specimen finally gave way; then these rapidly increased in number, splitting the glass into innumerable prisms, which finally bent or broke, and the specimen was destroyed

The following Table gives the results of the experiments upon the cut cubes of glass:—

	Mean resistance to crushing	
	in pounds	in tons
Flint-glass	13,130	6·861
Green glass	20,206	9·010
Crown-glass	21,867	9·762

Hence, comparing the results on cylinders with those on cubes, we find a mean superiority in the former case in the ratio of 1·6 : 1, due to the more perfect annealing of the glass.

On the Resistance of Glass Globes to internal pressure.—In these experiments the tenacity of glass is obtained by a method free from the objections to that before detailed. Glass globes, easily obtained of the requisite sizes, in a nearly spherical form, were subjected to an internal pressure obtained by means of a hydraulic pump, uniformly and steadily increased till the globe gave way. The lines of fracture radiated in every direction from the weakest part, passing round the globe as meridians of longitude and splitting it up into thin bands, varying from $\frac{1}{10}$th to $\frac{1}{8}$th of an inch in breadth.

The following Table gives the results of the experiments on the resistance of glass globes to internal pressure:—

Description of glass	Diameters	Thickness	Bursting pressure per square inch
	Inches	Inches	Pounds
Flint-glass	4·0 × 3·98	0·024	84
	4·0 × 3·98	0·025	93
	4·0	0·038	150
	4·5 × 4·55	0·056	280
	5·1 × 5·12	0·058	184
	6·0	0·059	152
Green glass	4·95 × 5·0	0·022	90
	4·95 × 5·0	0·020	85
	4·0 × 4·05	0·018	84
	4·0 × 4·03	0·016	82
Crown-glass	4·2 × 4·35	0·025	120
	4·05 × 4·2	0·021	126
	5·9 × 5·8	0·016	69
	6·0 × 6·3	0·020	86

The formula which expresses the relation of the bursting pressure to the thickness and diameter of the globe, is—

$$P = \frac{aT}{A},$$

where a = the longitudinal sectional area of the material in square inches, that is in the line of rupture or line of *minimum* strength, A = the longitudinal sectional area of the globe in square inches, and T = the tenacity of the glass in pounds per square inch. Hence from the above experiments we deduce—

Pounds
T=4200 for flint-glass,
=4800 for green glass,
=6000 for crown-glass
5000=mean tenacity of glass

Here the mean tenacity is nearly twice that obtained in the experiments upon thick bars, a result, which perhaps corresponds with the difference between the crushing strength of cylinders and cubes, and is largely attributable to the condition of annealing.

On the Resistance of Glass Globes and Cylinders to an external pressure.—The manner of conducting these experiments did not differ in any essential detail from that pursued in the experiments upon wrought iron. The globes and cylinders, after having been hermetically sealed in the blowpipe flame, were fixed in a wrought-iron boiler communicating with a hydraulic pump. In this position an increasing pressure was applied until the globes broke, the amount of pressure at the time being noted by means of a Schäffer pressure gauge. During the collapse the globes were reduced to the smallest fragments, so that no indication of the direction of the primary lines of fracture could be discovered.

The following Table contains a summary of the results on glass globes subjected to an external pressure.—

Description of glass	Diameters	Thickness	Collapsing pressure per square inch
	Inches	Inches	Pounds
Flint-glass	5·05×4·76	0·014	292
	5·08×4·7	0·018	410
	4·95×4·72	0·022	470
	5·6	0·020	475
	8·22×7·45	0·010	35
	8·2 ×7·2	0·012	42
	8·2 ×7·4	0·015	60
	4·0 ×3·98	0·024	(900*)
	4·0	0·025	(900*)
	6·0	0·059	(1000*)
Green glass	5·0×5·02	0·0125	212

The following Table contains a similar summary of the results upon cylindrical vessels —

Description of glass	Diameters	Length	Thickness	Collapsing pressure per square inch
	Inches	Inches	Inches	Pounds
Flint-glass	3·09	14·0	0·024	85
	3·08	14·0	0·032	103
	3·25	14·0	0·042	175
	4·05	7·0	0·034	202
	4·05	7·0	0·046	380
	4·06	13·8	0·043	180
	4·02	13·8	0·064	297
	3·98	14·0	0·076	382
	4·05	7·0	0·079	(500†)

The paper includes an investigation of the laws of collapse as exhibited in these results, and the following general formulæ are obtained :—

For glass globes $$P = 28,300,000 \times \frac{k^{1\cdot4}}{D^{3\cdot4}},$$

For glass cylinders $$P = 740,000 \times \frac{k^{1\cdot4}}{D\,L},$$

where P = the collapsing pressure in pounds per square inch, k = thickness in inches, D and L = diameter and length respectively in inches.

* These globes remained unbroken

† Remained unbroken

These are the general formulæ for glass vessels subjected to an external pressure, and the latter is precisely similar to that found for sheet-iron cylinders

Transverse Strength of Glass —The authors derive the general formula

$$W = 3140 \times \frac{K\ D}{l},$$

where W = breaking weight in pounds, K = area of transverse section, D = depth of section, l = length between supports ,—to express the transverse strength of a rectangular bar of glass supported at the ends and loaded at the middle

II “On the Atomic Weight of Graphite”

By BENJAMIN C. BRODIE, Esq , F.R S., Pres C S , Professor of Chemistry in the University of Oxford Received May 5, 1859.

(Abstract)

In this paper the author arrives at the following results —That carbon in the form of graphite forms a system of peculiar compounds, different from any compounds of carbon yet known, and capable of being procured only from graphite That graphite, within certain limits, functions as a distinct element, capable indeed of being converted by certain processes of oxidation into carbonic acid and thus identified with the other forms of carbon, but having a distinct atomic weight, namely 33 ($H=1$)

After the detail of certain experiments by which the author was led to believe in the existence of a distinct system of compounds of graphite, an account is given of a peculiar crystalline substance formed by the prolonged oxidation of graphite This substance consists of transparent plates of a pale yellow colour, which exhibit under the microscope an appearance distinctly crystalline. The analysis of this substance gave for its formula $C_{11}\ H_4\ O_5$ ($C=12$, $O=16$). From the ratio of the hydrogen to the oxygen in this substance, from the circumstance that it is procured from graphite alone, and from its general physical properties, it is inferred that this substance is the term in the system of graphite which corresponds to the compound of silicon, oxygen and hydrogen, in the system of silicon, procured by Wöhler from the graphitoidal form of that element, and to which Wohler has assigned the formula $Si_4H_4O_5$ ($Si=21$). If it be assumed that this conclusion is correct, the further inference is that the bodies are similarly constituted. On this hypothesis, to arrive at the atomic weight of graphite, the total weight of carbon, 132, is to be divided by 4, which gives the number 33, and for the formula of the body, putting $Gr=33$, we have $Gr_4H_4O_5$.

This conclusion is confirmed in a remarkable manner by the specific heat of graphite. The specific heat of the elemental bodies varies inversely with their atomic weight. This law is so well established, that Regnault has even proposed to determine the atomic weight by it exclusively. There are, at any rate, only two numbers which can be assigned as the product of the specific heat into the atomic weight of the elemental bodies, namely, approximately the numbers 3.3 and 6.6. But to this law there is one singular exception. Carbon in all its forms is anomalous. The specific heats of diamond, graphite, and wood-charcoal are each different, and taking the atomic weight of carbon as 6 or 12, no one is accordant with the law. The specific heat of graphite is 0.20187. Now, taking the atomic weight of graphite as 33, we have $33\times201=6.63$, a result in accordance with the law. The inference is, that the assertion that 33 is the atomic weight of graphite is not only a convenient expression of chemical analysis, but corresponds to a physical fact.

May 19, 1859.

Major-General SABINE, R.A., Treas. and V.P., in the Chair.

Professor Henry Darwin Rogers was admitted into the Society.

The following communications were read:—

I. "On the Alloys."—Part I. "The Specific Gravity of Alloys." By A. Matthiessen, Ph.D. Communicated by Prof. Wheatstone. Received May 17, 1859.

(Abstract.)

Before commencing a research into the electric conductivity of alloys, the author deemed it requisite, as a preliminary step, to

determine their specific gravities, and the methods employed and results obtained in this inquiry are given in the present paper

The metals used were antimony, tin, cadmium, bismuth, silver, lead, mercury, and gold The silver and gold were obtained in a state of purity from the refiners, the other metals were purified by methods which are described The quantity prepared of each alloy was twenty grammes The fused alloys were cast in a form and very thin, to avoid internal cavities from crystallization Three separate determinations were made of their specific gravity, which, together with the mean result, are given in Tables In every case the alloy was recast at least three times before the first determination, and once again before each succeeding one The distilled water used in the weighing was first boiled and allowed to cool *in vacuo*, and the alloys were suspended in it by a fine platinum wire, except the soft amalgams, which were weighed in a tube similarly suspended In calculating the specific gravities, the weight of water displaced was corrected for the temperature, the unit in all cases being distilled water at 0° C All the weighings were reduced to a vacuum, and a correction was made for the platinum wire which dipped in the water.

The numerical results are stated in three Tables, of which the first gives the specific gravities of the pure metals employed, and the temperature in Centigrade degrees, the second gives the same of the alloys, and the third exhibits the mean specific gravities found, and the specific gravities as calculated,—1, from the volume of the metals forming the alloy, 2, from their equivalent, and 3, from their weight

From the last Table it appears that the alloys of antimony are greater in volume than the aggregate of the constituent metals, while those of bismuth, silver, and gold are less The following alloys expand greatly on cooling, viz all those of bismuth-antimony, bismuth-gold, and bismuth-silver, which were experimented on, those of bismuth-tin, from Bi_6 Sn to Bi_3 Sn, the rest of the series very slightly, and bismuth-lead, viz. Bi_6 Pb and Bi_4 Pb (Bi_2 Pb slightly), the rest apparently not at all. Of the bismuth cadmium series, Bi_6 Cd and Bi Cd_4 expand very slightly, the rest not at all The zinc-alloys are all so very crystalline that no results of value were obtained respecting them

In making the determinations given in the paper, the author was assisted by Dr M Holzmann and Mr C Long.

II "On the Anatomy of *Victoria Regia*" Part II By Arthur Henfrey, Esq, F.R S, F L S. &c, Professor of Botany in King's College, London Received May 5, 1859

(Abstract)

This paper is a continuation of one published in the Philosophical Transactions for 1852 (p 289), and discusses the general question of the anatomical structure of the stems of Monocotyledons and Dicotyledons, especially in reference to some objections taken against the author's views respecting the stems of the Nymphæaceæ Certain peculiarities of the structure of roots are next examined, and these are shown to be formed on the Dicotyledonous type in *Victoria*

The germination of the seed is described in a manner differing to some extent from the accounts given by Planchon, Trécul, and Hooker The error of Trécul, in stating that the earlier leaves are devoid of a stipule, is shown to depend upon his overlooking the true axillary position of that organ

The Phyllotaxy is next treated, with the development and arrangement of the leaves and roots, lastly, a complete history of the development of the flower, showing that the apparently inferior position of the ovary depends upon a great enlargement of the receptacle after the formation of the various organs forming the flower

III. "On the Conductivity of Mercury and Amalgams." By F. Crace Calvert, Esq., and R. Johnson, Esq Communicated by Professor Stokes, Sec R S. Received April 14, 1859

(Abstract)

The object of the researches described in this paper, was to carry out with reference to amalgams the investigations relative to alloys contained in a former paper In comparing the results of theory and experiment in the manner followed in the former paper, the conducting power of mercury itself was a constant, which it was essential to know The figure given in the former paper was mercury=677, on the scale silver=1000 On adopting in the first instance this value of the conducting power of mercury, the results obtained with alloys, which consisted mainly of mercury, appeared very anomalous, it seemed as if a very small per-centage of even the best conducting metals reduced immensely the conducting power of mercury But it was suggested to the authors, that the apparently high conducting power of mercury obtained by their method, was probably due to the transference of heat by convection, that the real conducting power of mercury for heat was low, like its conducting power for electricity, that the other metal, contained in small quantity in the amalgam, acted by rendering the amalgam viscous, and thereby interfering with the transference of heat by convection, and that the low conducting power of mercury would show itself on merely inclining the vessel used in the experiment, so that the box containing the warm water should be higher than the other Experiment confirmed this view As the apparent conducting power of mercury was found continually to decrease with an increase in the inclination of the vessel, it was found necessary, in order to obtain correct results, to arrange so that the bar-shaped box containing the mercury or fluid amalgam was actually vertical in the experiment. In this way the authors obtained for mercury the figure 54, on the same scale as before It is worthy of remark, that mercury comes out the worst conductor of all the metals tried, the figure for bismuth, which had previously been the lowest, being 61. This is in analogy with water, also a fluid, the conducting power of which is known to be excessively low The conducting power of the more fluid amalgams determined by experiment with the box vertical, proved to be in all cases nearly the same as that of pure mercury, in conformity with the law mentioned by the authors in their former paper, that alloys in which there is an excess of the number of equivalents of the worse conducting metal, over the number of equivalents of the better conductor, do not sensibly differ in conducting power from the worse conductor alone In the case of amalgams generally, the conducting power obtained by experiment was found to agree pretty closely with the number calculated from the per-centages and conducting powers of the component metals.

In conclusion, the authors give some further experiments on the conduction of heat by compound bars, formed of metals placed in some cases end to end in alternate cubes, in other cases side by side in parallel bars, extending the whole length of the compound bar

Among bars of the latter kind, it was found that it was only in the case of bismuth and antimony that the compound bar conducted heat according to the calculated amount

May 26, 1859

Sir BENJAMIN C BRODIE, Bart, President, in the Chair

The following communications were read —

I. "On the Intimate Structure, and the Distribution of the Blood-vessels, of the Human Lung" By A T H Waters, Esq, Lecturer on Anatomy and Physiology, Liverpool Communicated by Dr. Sharpey, Sec R S. Received April 7, 1859

Having been recently engaged in investigating the anatomy of the human lung, I beg to lay before the Royal Society some of the results of my observations with respect to the arrangement of the ultimate air-tubes and the distribution of the blood-vessels of the organ

The bronchial-tubes of the lungs, after several divisions and subdivisions, which for the most part are of a dichotomous nature, terminate in a dilatation, into which open a number of elongated cavities, which constitute the ultimate expressions of the air-tubes. These elongated cavities, to which various names have been given, I propose to call *air-sacs*, as being, in my opinion, more appropriate to their shape and arrangement than any term hitherto used, and the series of air-sacs connected with the extremity of each bronchial twig, with its system of blood-vessels, &c, I shall call a *lobulette*

Every lobule of a lung is composed of a number of lobulettes, and thus the description of a single lobulette will suffice for that of the entire lobule

Each lobulette consists of a collection of air-sacs, which vary in number from six to eight, ten or twelve. The air-sacs are somewhat elongated cavities, communicating with the dilated extremity of a bronchial tube by a circular opening, which is usually smaller than the sac itself, and has sometimes the appearance of a circular hole in a diaphragm, or as if it had been punched out of a membrane which had been stretched across the entrance to the sac When this is the case, the sac dilates suddenly beyond the opening The sacs of the lobulette are placed side by side, and are separated from each other by thin membranous walls Their shape, when properly inflated, or when distended by some material which has set in them, as gelatine, or a mixture of wax and turpentine, is polygonal They approach the circular form, but in consequence of their mutual pressure, the parietes become somewhat flattened The sacs increase slightly in size as they pass from the bronchial tube to their fundus, the latter being usually the broadest part of the sacs, but they often have an almost uniform diameter throughout All the sacs pass from the extremity of the bronchial tube *towards* the circumference of the lobule of which the lobulette forms a part, they consequently radiate from the tip of the bronchial twig The sacs connected with one lobulette do not communicate with those of another lobulette As the sacs pass towards the boundary of the lobulette, they often bifurcate, and here and there circular orifices exist, leading to smaller air-sacs, sometimes only to a small group of "air-cells," or *alveoli.* If we trace the air-sacs from their fundus, we may say that, passing from the periphery of the lobulette, and diminishing somewhat in size, they all terminate in the dilated extremity of the bronchial tube, as they thus proceed they often join two or three together, and these terminate in a single mouth The tube which results from the union of two or more sacs, is smaller in capacity than the sacs taken together, but greater than either of them individually The dilated extremity of the bronchial tube above alluded to constitutes the "point de réunion" of all the air-sacs, and may be considered as the common centre of the lobulette

The walls of which the air-sacs are composed are exceedingly thin, and much sacculated, *i e* they have in them a number of small, shallow, cup-like depressions, separated from each other by portions of membrane which are more or less raised and project into the interior of the sac The bottom of the air-sac presents the same appearance as the lateral walls, and the cup-like depressions, or *alveoli*, are there very numerous The number of these alveoli varies very much; I have counted as many as ten at the fundus of an air-sac in a cat's lung, in the human lung I have counted five and six, but the number is not usually quite so great Close to the bottom in some of the air-sacs in the human lung, a circular opening, similar to those already alluded to as leading to other sacs, small and constricted, is often seen, and has the appearance as if it led to another sac, on examination, however, it will be found to be produced by a projection inwards of the membrane of the sac, and to lead to a small cavity, or group of alveoli

The number of alveoli existing in the air-sacs varies—Rossignol states that each "infundibulum" (air-sac) contains from ten to twenty alveoli My own observations entirely accord with this statement I have found the number varying from eight to twenty

The air-sacs externally, by their fundus, rest on the pleura, but within the substance of the lung they in part rest on, and are supported by, the bronchial tubes and blood-vessels

The air-sacs are separated from each other by thin walls, the membrane composing which, in a lung inflated and dried, is very transparent The projection of this membrane in the shape of a thin process, having a sharp margin, constitutes the septa between the alveoli, and wherever an opening exists leading into a smaller sac, this membrane projects in a similar way, and forms a circular orifice which is much smaller than the cavity to which it leads ,—the sac, in fact, dilates abruptly on the distal side of the opening. It is in the membrane composing these walls, and in the septa of the alveoli, that the capillaries of the pulmonary artery are spread out

The number of air-sacs belonging to a lobulette varies I have counted as many as six communicating with a bronchial tube incised horizontally, so that probably only half the sacs were left, this, however, is a larger number than is usually found, from six to eight or ten is the more common number

Each lobulette is separated from those by which it is surrounded, by walls which appear to resemble in every way the walls of the air-sacs, and in an adult inflated and dried lung, careful observation is necessary to make out the partitions. That perfect septa do exist, is proved by laying open, in a recent lung, a bronchial tube to its ultimate division, when by placing a fine blowpipe in it, and blowing down it, a single lobulette is alone inflated

The separation of the lobulettes is further distinctly perceptible in the recent lungs of infants, in which the line of demarcation between the lobulettes is often plainly seen, on the surface

The observation of the fœtal lung, however, affords most satisfactory evidence of the separation of the lobulettes, and tends to confirm the views here taken of the arrangement of the ultimate air-tubes

The recent lung of a full-grown fœtus presents on its surface no appearance of air-sacs, or vesicles, or cells, but if it be inflated, it will present different appearances, according as the inflation has been partial or entire In a portion of lung only partially inflated, a number of tubes will be seen terminating beneath the pleura in cæcal extremities, their light colour contrasting strongly with the surrounding dark-coloured tissue The exact arrangement of these tubes may be sometimes seen They will be found to exist in groups or clusters, and are seen to pass from the cæcal ends to a point, in which they terminate, and where they all appear to join, or, to describe them in the inverse direction, they pass from a point at some distance from the surface, and radiate towards the pleura The tubes are seen to have numerous constrictions and bulgings, they terminate in extremities rounded, or nearly so In a preparation of this kind it is often easy to see the bronchial tube for a short distance before it terminates, and not only is the terminal group of air-sacs (lobulette) visible, but two or more of the previous ones arising laterally from the bronchial tube may be also seen The uninflated lung-substance lying between the distended air-sacs is distinctly seen when there has been only slight inflation, and the isolated condition of each group of sacs is very apparent, the sacs passing from different points are seen radiating in different directions In a lung which has been fully inflated, the grouped appearance is lost, and the ordinary condition of the distended lung is observed.

If, in a fœtal lung, we follow out a bronchial tube, we find that the smaller branches of the tube have connected with them clusters of little pyriform red-coloured bodies, which look very much like a number of grapes attached to their stalks In a fœtus of six months I have found it somewhat difficult to separate each individual body, but in a full-grown fœtus there is no difficulty in doing so, each little body is attached to a short pedicle If air be blown down a bronchial tube leading to the exposed bodies, the latter become distended

The little bodies just described are the ready-formed groups of air-sacs, or lobulettes, the pedicle with which each is connected is the terminal bronchial twig The lobulette thus formed is surrounded by its sheath, and no communication exists between it and adjoining ones

That the appearances I have described in the artificially inflated fœtal lung are not the result of any abnormal distension, I have been able to prove by observing the same appearances in the lung of a child in which only partial respiration had taken place

Do the air-sacs communicate with each other by any orifices except that by which they communicate with the bronchial tube?

Different opinions have been expressed on this point Adriani states that he has observed such orifices, and specially mentions that they are most clearly to be seen in the stag Dr Thomas Williams considers that the "intercellular passages" intercommunicate, and are perforated by secondary passages at every point Rossignol, Schultz, Mandl, and Milne-Edwards, deny the existence of such communications. From observations, made with much care and frequently repeated, I have satisfied myself that the opinion expressed by the latter authors is correct. I have never found, either in the lung of man or in that of the dog, cat, pig, sheep, or any other mammal I have examined, any lateral orifices of communication between the sacs of a lobulette

Alveoli of the Bronchial Tubes —The termination of the bronchial tubes has a special character, first pointed out by Rossignol He says, "In the bronchial divisions of the two last, and sometimes three last orders, it is plainly seen, when they are opened longitudinally, that their surface is covered over, or, as it were, honeycombed with a number of small, regular, shallow cavities, placed side by side, and separated by thin perfect walls of the same height, which project into the interior of the bronchial tube" The existence of these bronchial alveoli has been noticed by subsequent observers, they may be easily seen in a lung injected and inflated, and sometimes even in one which has been simply soaked in spirit for a few days, they resemble the alveoli of the air-sacs, they are best seen in the lungs of some of the lower animals, as the cat, in which they are found in the ultimate bronchial tubes and their dilated extremities. In man I have never seen them, except at the extremity of the tubes, and in many lungs I have found no appearance of them at all—they appear to become obliterated with advancing age. In the infant I have found them in the last divisions of the bronchial tubes, and their dilated extremities, but not in the penultimate or earlier branches, and even in the last divisions they are not always present previous to the dilatation In respect of the extent, therefore, to which these alveoli exist in the human lung, my observations do not quite accord with those of Rossignol

The Blood-vessels of the Lungs —The branches of the pulmonary artery accompany the bronchial tubes, arrived at the extremity of which, they give off branches to the bronchial alveoli, and terminate in vessels which take their course in the walls of the air-sacs in no very definite or regular manner From these vessels the pulmonary plexus arises

The pulmonary veins, receiving the blood from the plexus of the air-sacs, pass from the periphery of the lobulettes, and running in the spaces between the lobules, make their way, independently, to the root of the lung

The pulmonary plexus is situated in the walls which separate the air-sacs, in the septa of the alveoli, and around the margins of the openings which exist in the sacs The plexus consists of a single layer of vessels, which, as already pointed out by Mr Rainey, is in no instance doubled on itself In the septa of the alveoli, and in the margins of the orifices alluded to, the plexus does not reach quite to the free border of the membrane composing them

There is no distinct and separate vessel for each alveolus, but the branches of the terminal artery take their course along the walls of the air-sacs, and give off branches which for the most part run in the septa of the alveoli, some of them, however, pass across the alveoli From these vessels, and from the branches first mentioned, the capillary plexus arises The plexus, when formed, maintains a tolerably uniform size throughout In a well-injected preparation, inflated and dried, it will be seen that the spaces between the vessels are somewhat greater in diameter than the vessels themselves

The branches of the pulmonary artery do not anastomose till they reach the termination of the bronchial tubes, on the air-sacs they anastomose freely. It is somewhat difficult to decide whether the vessels of one lobulette anastomose with those of another. On looking at a preparation injected and dried, it seems as though the septum, separating one lobulette from another, resembled in every respect the walls of the air-sacs, but as the lobulettes are originally separate and independent bodies, as seen in the fœtal lung, it is probable that the vessels of each are distinct If so, there must be, where the walls of two lobulettes are in contact, two layers of capillaries lying side by side, and from the mode of formation of the lobulettes, and from the fact that I have been able, in some preparations of the adult lung, partially to separate the lobulettes from one another, I believe that their vessels are distinct, that they terminate in their proper radicle-vein, and that thus the capillaries on the outer wall of the lobulette are only exposed on one side to the atmosphere

The Bronchial Vessels —It has long been held that the bronchial arteries are distributed to the air-tubes, the areolar tissue, and the vessels of the lungs, and that they pour their contents *partly* into the pulmonary veins, and *partly* into certain deep bronchial veins, which have been described by most anatomists as accompanying the arteries within the lungs An opinion has also been entertained that a communication exists between the bronchial vessels and the branches of the pulmonary arteries Without referring to the experiments and results of other observers, I proceed to state my own

My observations have been made on the lungs of the cat, the dog, the rabbit, the pig, the calf, and the sheep, as well as on those of man The following remarks have special reference to the results obtained in the human lung.

When the pulmonary artery is injected so that the fluid reaches the pulmonary plexus but does not pass to any extent into the pulmonary veins, the blood-vessels of the bronchial mucous membrane and of the other portions of the bronchial tubes *never* become injected. When, however, the injection is continued so as to fill the pulmonary veins, the vessels of the bronchial tubes become partially injected

When the pulmonary veins are injected, whether the pulmonary plexus be well-filled or not, the vessels of the bronchial tubes and of the bronchial mucous membrane are always injected The bronchial tubes are often seen to be injected when the pulmonary plexus is only very partially so, the fluid seeming to find its way from the pulmonary veins into the vessels of the bronchial tubes more readily than into the capillaries of the air-sacs

When a bronchial artery, as it enters the substance of the lung, is injected, the vessels of the bronchial tubes become filled—both those of the mucous membrane and of the deeper portions—and the fluid finds its way into the pulmonary veins If the injection be continued, it is easy to inject the pulmonary plexus through the medium of the bronchial arteries, and injection is often found in the branches of the pulmonary arteries.

In injecting a bronchial artery in the human subject, I have always found that part of the bronchial tubes nearest the point of insertion of the injection pipe more fully injected, both as regards the mucous membrane and the deeper structures, than the parts situated towards the extremity of the tubes I have found the larger branches injected nearly to the end of the tubes, but not the fine vessels of the mucous membrane This seems to me to be due to the fact, that throughout the entire extent of the tubes there is so free a communication between the bronchial vessels and the pulmonary veins, that the fluid finds its way into the latter more readily than into the fine plexus of the extreme tubes

The Bronchial Veins —The examination of a large number of specimens, both of the lungs of man and the lower animals, and the injection of the vena azygos on several occasions, have not enabled me to find the so-called deep bronchial veins, as *venæ comites* of the bronchial arteries I have always found a small vein or veins, generally a single one, situated at the posterior aspect of the bronchus, and terminating, as shown by the injection, in the structures of bronchus, lower part of trachea, and glands at the root of the lung, but not passing along the bronchial tubes *within* the lung, and therefore being in no way concerned in returning the blood distributed to those parts by the bronchial arteries

A piece of human lung well injected by the bronchial artery, exhibits on the mucous membrane of the bronchial tubes, a fine plexus of vessels, taking for the most part a longitudinal direction, under these other vessels are found, which run transversely beneath the elastic tissue, in the direction of the muscular fibres, these deeper vessels are larger than the superficial ones, there is a distinct communication between the two sets

I now pass to consider *in what manner*, and *where*, the communication which undoubtedly exists between the pulmonary and bronchial vessels, takes place

If the statement I have made with reference to the bronchial veins be true, those vessels do not return the blood supplied by the bronchial arteries, except so far as the latter vessels distribute it to the bronchi themselves and the structures about the root of the lung, and the next point we have to examine is whether any communication exist between the bronchial arteries and the pulmonary arteries In all the best injections I have made, whether in man or the lower animals, in which the injection was introduced by the pulmonary artery, I have never found the vessels of the bronchial tubes injected unless the pulmonary veins were well-filled, and in such specimens I have *seen* vessels passing from the bronchial tubes, and joining a branch of a pulmonary vein I have found branches of the pulmonary artery containing injection, when the latter has been introduced through the bronchial artery, but in such specimens I have found portions of the pulmonary plexus injected, and I believe the injection has found its way through the plexus into the pulmonary arteries I have never seen the vessels described by some anatomists as passing from the pulmonary artery to the bronchial tubes

With regard to the communication of the bronchial vessels with the pulmonary veins, it admits of ocular proof It is unquestionably more easy to inject the vessels of the finer bronchial tubes through the pulmonary veins, than through the bronchial arteries at the root of the lung, it is also possible to inject those vessels to a certain extent through the pulmonary veins, without injecting the pulmonary plexus On the other hand, the injection thrown in through the bronchial arteries rapidly and readily finds its way into the pulmonary veins. These facts seem to prove that the blood distributed by the bronchial arteries within the lung is poured into the pulmonary veins, and that the whole vascular system of the bronchial tubes communicates with the same veins

It has been stated by Mr. Rainey, that the vessels of the bronchial tubes anastomose at the extremity of the tubes with the vessels of the "air-cells" (air-sacs) In this I cannot concur, for I have never been able to fill these extreme vessels by injection through the pulmonary artery My belief is, that at the termination of the tubes, as elsewhere, the blood of the bronchial arteries is poured into the pulmonary veins.

Dr Heale has advanced the opinion that the bronchial arteries do not supply the bronchial mucous membrane at all, and that they neither communicate with the pulmonary arteries nor veins. My observations have given results entirely opposed to this view.

With reference to the view taken by Adriani, and subsequently adopted by Dr. Thomas Williams, that the vessels of the bronchial mucous membrane terminate in the pulmonary veins, and those of the deeper plexus in the bronchial veins, it is not borne out by the experiments I have made, which appear to prove that not only do the same vessels supply the superficial and deep plexuses of the tubes, but that both plexuses discharge their contents into the same receptacles.

II. "On Certain Sensory Organs in Insects, hitherto undescribed." By J. BRAXTON HICKS, M.D. Lond., F.L.S. &c. Communicated by JOHN W. LUBBOCK, Esq. Received May 14, 1859.

(Abstract.)

The author commences with an allusion to papers published in the Linnean Society's 'Journal' and 'Transactions' respecting groups of organs, abundantly supplied with nerves, on the bases of the halteres of Diptera, also on the nervures of the wings and on the elytra of Coleoptera, and now gives a drawing which shows forth these organs, and the nerve proceeding to them on the halteres. He then describes, for the first time, somewhat similar organs on the apices of the palpi of some Diptera, and on their base in many Hymenoptera, as *Apis, Vespa, Nomada, Megachile, Bombus*, &c. These are well shown in the *Vespa Crabro*, or Hornet, where the nerve is seen expanding in the thin membrane which covers in the opening beneath in the wall of the member.

In the paper also, it is pointed out for the first time, that on the apex of the palpi of Lepidoptera there is invariably found a structure which is more or less of a cavity, generally tubular, and sometimes extending inwards nearly the length of the last segment, but sometimes only a depression To it a nerve is given which expands on the apex of the cavity

The author then describes groups of organs, allied in form to those on the palpi, which are to be found on the legs of all insects yet examined There are about three groups situated about the trochantero-femoral joint, and to them nerves can be distinctly seen proceeding, and in *Meloe* the branch is seen to pass up the opening in the wall, to terminate in a papilla in the centre of the membrane covering it in

It is also shown that the bladder-like apex of the palpi, instead of being smooth, as is generally described, is covered with a great number of small bodies, something in form like ninepins, sometimes exceedingly small, requiring a $\frac{1}{8}$-inch objective to make them out, when they can clearly be discerned to be a modified condition of true hairs, copiously supplied with nerves The author names these "*tactile hairs*," and points out their existence in all palpi used for touching, and in other organs subservient to that function. These tactile hairs are very large in the palpi and antennæ of *Dyticus marginalis* The barrel-like organs of the Lepidoptera are next investigated, and are shown to have a nerve passing up them, but whether proceeding to the apex of the nipple-like papilla on them or not, cannot be quite made out They are pointed out as being nearly allied to the organs on each of the palpi of the Earwig (*Forficula auricularia*).

The author refers to the sacs found on the antennæ of all insects, which have been fully treated of in two papers read by him before the Linnean Society, and published in their 'Transactions,' and he lastly examines the probable functions of all these organs, which must be of sensation, probably special.

Attention is also called to the value of bleaching the tissues by chlorine in investigating the structure of insects, which process was first used by the author and described by him in the papers above mentioned.

About forty drawings accompany the paper illustrative of the structures described

III "On Lesions of the Nervous System producing Diabetes." By Frederick W Pavy, M D Lond &c. Communicated by Dr Sharpey, Sec R S. Received May 17, 1859.

(Abstract)

The author commenced his paper by stating, that all the experiments he had performed since his communication on the "Alleged Sugar-forming Function of the Liver" had been placed in the possession of the Royal Society, had confirmed the conclusions he had there arrived at As far as his knowledge extended, it might be said that in the healthy liver during life there is a substance which he had spoken of under the term of hepatine, and which possesses the chemical property of being most rapidly transformed into sugar when in contact with nitrogenized animal materials In the liver after death this transformation takes place, but in the liver during life there seems a force or a condition capable of overcoming the chemical tendency to a saccharine metamorphosis

Experiments are mentioned to show that when the medulla oblongata is destroyed, and the circulation is maintained by the performance of artificial respiration, the sugar formed in the liver as a *post-mortem* occurrence is distributed through the system, and occasions the secretion of urine possessing a strongly saccharine character

Although the destruction of the medulla oblongata leads to this effect, yet division of the spinal cord, which has been practised as high as between the second and third cervical vertebræ, has not been attended with a similar result The brain (cerebrum) has also been separated from the medulla oblongata by section through the crura cerebri, and from the results of the experiments in which this operation has been performed, Dr Pavy believed that the functions of the brain may be completely destroyed, without placing the liver in the condition noticeable after actual death, or after lesion of the medulla oblongata. On account of the accidental disturbances,—such as implication of the medulla oblongata, possibly by concussion, obstruction of the respiration, and the effects of the great loss of blood sometimes attending division of the crura cerebri,—the interpretation of the result is rendered a little difficult. In an experiment, which proved most conclusive, performed to corroborate the author's previous observations

whilst his communication was being written, there were none of these disturbing circumstances In a healthy dog, during a period of digestion, the crura cerebri were completely divided. The animal was thereby thrown into a state of unconsciousness, but breathed efficiently of its own accord The urine in an hour and a quarter's time was found perfectly free from sugar

After poisoning by strychnine, the effect is the same as after destruction of the medulla oblongata The circulation being maintained by artificial respiration, the urine becomes strongly saccharine

Looking to these facts, and to the effect of Bernard's puncture of the fourth ventricle in producing diabetes, the author is led to regard the medulla oblongata as a centre, either directly presiding over the functional activity of the liver, or indirectly affecting it by altering through the medium of another or other organs the condition of the blood going to it, and he has endeavoured to establish upon positive grounds the channel by which the propagation of the nervous influence may take place It was this line of research that conducted to the discovery of the strongly diabetic effect produced by dividing certain parts of the sympathetic

The medulla oblongata being thus presumed to form a centre giving to the liver a force which prevents the saccharine metamorphosis of its hepatine, experiment had already shown that it cannot be through the spinal cord or the pneumogastrics separately, that the transmission of nervous influence takes place But an experiment was performed to determine the effect of dividing both the spinal cord and the two pneumogastrics together The cord was crushed between the third and fourth cervical vertebræ, and about half an inch of each pneumogastric was cut away from the centre of the neck Artificial respiration was performed to keep up the circulation The urine remained entirely free from sugar, and the liver was found in an exsaccharine state at the moment of discontinuing the respiration, and became strongly saccharine afterwards.

On next dividing all the nerves in the neck, an operation effected by performing decapitation, the result that followed after three quarters of an hour's artificial respiration was strongly saccharine urine After this experiment, and that of division of the spinal cord and pneumogastrics, reason was afforded for looking to the sympathetic, and from the experiments that have been made and are described, the following conclusions have been arrived at. The animal selected for observation has been the dog, subsisting upon an animal diet, and operated upon at a period of full digestion.

"Division, on both sides of the neck, of the ascending branches of the superior thoracic ganglion which run up towards the canal formed by the foramina in the transverse processes of the vertebræ, for the vertebral artery, occasions an intensely marked diabetes. The urine has been found most strongly saccharine within even half an hour after the operation. The diabetic condition is only of a temporary character, passing off by the next day, and fatal pleurisy is always induced.

"Division of the ascending branches of the superior thoracic ganglion on one side of the neck only, has occasioned merely the presence of a trace of sugar in the urine in an hour and a half's time. The same operation then performed on the other side has produced in half an hour's time an intensely saccharine urine.

"*Carefully* ligaturing the two vertebral and the two carotid arteries does not lead to saccharine urine, but when the carotids have been tied, and the tissue in connexion with the vertebrals before their entrance into the canals is a little roughly treated, without however dividing the larger sympathetic filament ascending from the superior thoracic ganglion, the urine is rendered rapidly and strongly saccharine.

"Division of the sympathetic filaments that have entered the canals does not alone produce diabetes, but if the contents of these canals be divided, and the carotid arteries at the same time ligatured, saccharine urine is the result.

"This result is produced when the contents of the osseous canals are divided as high as the second cervical vertebra. It has also arisen after destroying the structures in the neighbourhood of the vertebral foramen on the posterior surface of the transverse process of the atlas, but has not yet been noticed after a similar operation on the anterior surface of the process.

"Dividing the contents of the canals and the tissue in immediate contact with the carotid vessels has not produced diabetes; but when the carotids have been afterwards tied, strongly saccharine urine has resulted.

"Of all the operations on the sympathetic of the dog that have yet been performed, removal of the superior cervical ganglion the most rapidly and strongly produces diabetes After the removal of one ganglion, the urine has been found intensely saccharine in an hour's time, and the saccharine character has remained during the following day, but has disappeared by the next Subsequent removal of the other ganglion a few days later has been followed in half an hour's time with a strongly marked diabetic effect, which, however, has been again only of a temporary nature

"Division of the sympathetic in the chest has been several times succeeded by saccharine urine In one case after division on one side only, the urine was intensely saccharine in half an hour's time On the other hand, many experiments have been made where both sides have been operated on, and only a merely traceable, or in a few instances, even no effect, has been noticeable

"In the rabbit, removal of the superior cervical ganglia, when the animal is in a strong and healthy state, is followed by diabetes, but the effect is not so rapidly produced as in the dog It has been noticed at the end of four hours after the operation, and has been observed to exist until the following day

"Excision of the superior cervical ganglia in the rabbit with a division of the pneumogastrics above their gangliform enlargement close to their exit from the skull, has been attended with the production of saccharine urine in a shorter space of time than when the ganglia alone have been removed, notwithstanding that division of the pneumogastrics in the situation referred to, has not been seen by itself to cause any positive effect"

Such is a simple statement of the principal conclusions derivable from the author's experiments, which are given in detail in his communication. As to the interpretation of the results that have been obtained, this he leaves for further investigation, in which he is now engaged, if possible, to disclose The experiments on the sympathetic were commenced under the notion that it might form the medium of transmission of nervous force from the medulla oblongata to the liver. From this supposition certain facts have been discovered which are left for the present, without discussing whether the notion that led to them is right or wrong

IV. "On the Electrical Condition of the Egg of the Common Fowl."

By JOHN DAVY, M.D., F.R.SS. L. & E. &c.

Received May 19th, 1859.

(Abstract.)

The structure of the egg suggested to the author the idea of its exerting electrical action. This was confirmed on trial. Using a delicate galvanometer and a suitable apparatus, on plunging one wire into the white, and the other, insulated, except at the point of contact, into the yolk, the needle was deflected to the extent of 5°, and on changing the wires, the course of the needle was reversed. When the white and yolk were taken out of the shell, the yolk immersed in the white, the effects on trial were similar, but not so when the two were well-mixed, then no distinct effect was perceptible.

Indications also of chemical action were obtained on substituting for the galvanometer a mixture consisting of water, a little gelatinous starch, and a small quantity of iodide of potassium, especially when rendered very sensitive of change by the addition of a few drops of muriatic acid. In the instance of newly-laid eggs, the iodine liberated appeared at the pole connected with the white; on the contrary, in that of eggs which had been kept some time, it appeared at the pole connected with the yolk, answering in both to the copper in a single voltaic combination formed of copper and zinc.

The author, after describing the results obtained, declines speculating on them at present, merely remarking, that in the economy of the egg, and the changes to which it is subject, it can hardly be doubted that electro-chemical action must perform an important part, and that in the instance of the ovum generally, i. e. when composed of a white and of a yolk, or of substances in contact, of heterogeneous natures.

V "On the Mode in which Sonorous Undulations are conducted from the Membrana Tympani to the Labyrinth, in the Human Ear" By JOSEPH TOYNBEE, Esq, F R S &c

Received May 24, 1859

The opinion usually entertained by physiologists is that two channels are requisite for the transmission of sonorous undulations from the membrana tympani to the labyrinth, viz the air in the tympanic cavity which transmits the undulations to the membrane of the fenestra rotunda and the cochlea, and secondly, the chain of ossicles which conduct them to the vestibule

This opinion is, however, far from being universally received, thus, one writer on the Physiology of Hearing contends that "the integrity of one fenestra may suffice for the exercise of hearing*," another expresses his conviction that "the transmission of sound cannot take place through the ossicula†," while Sir John Herschel, in speaking of the ossicles, says "they are so far from being essential to hearing, that when the tympanum is destroyed and the chain in consequence hangs loose, deafness does not follow‡"

The object of this paper is to decide by experiment how far the ossicles are requisite for the performance of the function of hearing

The subject is considered under two heads, viz —

1. Whether sonorous undulations from the external meatus can reach the labyrinth without having the ossicles for a medium

2. Whether any peculiarity in the conformation of the chain of ossicles precludes the passage of sonorous undulations through it

1. *Can sonorous undulations reach the labyrinth from the external meatus without the aid of the ossicles?*

This question has often been answered in the affirmative, apparently because it has been ascertained that in cases where two bones of the chain of ossicles have been removed by disease, the hearing power is but slightly diminished. In opposition to this view, it must, however, be remembered, that the absence of the stapes, or even its fixed condition (anchylosis), is always followed by total or nearly total deafness, and the following experiments, which demonstrate the great facility with which sonorous undulations pass from the air to a solid body, indicate that the stapes, even when isolated from the other bones of the chain, may still be a medium for the transmission of sound.

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* Mr Wharton Jones, Cyclopædia of Surgery, Art "Diseases of the Ear," p 23

† Lancet, 1843, p. 380

‡ Encyclopædia Metropolitana, Art "Sound," p 810

Experiment 1.—Both ears having been closed, a piece of wood, 5 inches long and half an inch in diameter, was held between the teeth, and a vibrating tuning-fork C having been brought within the eighth of an inch of its free extremity, the sound was heard distinctly, and it continued to be heard between five and six seconds.

Experiment 2.—One end of the piece of wood used in the previous experiment being pressed against the tragus of the outer ear, so as to close the external meatus without compressing the air contained within it, a vibrating tuning-fork C′ placed within a quarter of an inch of its free extremity, was heard very distinctly at first, and it did not cease to be heard for fifteen seconds.

Experiment 3.—Three portions of wood, of the same length and thickness as that used in the previous experiments, were glued together so as to form a triangle somewhat of the shape of the stapes, the base of this triangle being placed against the outer surface of the tragus, as in the previous experiment, the tuning-fork C′ vibrating within a quarter of an inch from its apex was heard for twelve seconds.

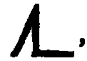
Considering, as shown by the above experiments, the great facility with which sonorous undulations pass from the air to a solid body, it may, I think, be assumed that the undulations in the tympanic cavity may be conveyed to the stapes even when this bone is isolated from the rest of the chain, and conducted by it to the vestibule; and when it is also considered that the absence of all the ossicles, or even a fixed condition of the stapes, is productive of deafness, there is strong evidence in favour of the opinion that *sounds from the external meatus cannot reach the labyrinth without the medium of the ossicles*.

2. *Is there any peculiarity in the conformation of the chain of ossicles which precludes the passage of sonorous undulations through it?*

This question has also been answered in the affirmative, on account of the various planes existing in the chain; and secondly, on account of the joints existing between the several bones composing this chain.

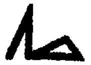
The following experiments refer to the influence of the varying plane of the bones forming the chain, and of its articulations, on the progress of sonorous undulations through it:—

I *Experiments illustrative of the influence of the variety of planes in the chain*

Experiment 1.—Three pieces of wood, each 5 inches in length and half an inch thick, were glued together thus , so as to represent the planes in which the malleus, incus, and stapes are arranged in the chain of ossicles, while three similar portions were glued end to end so as to form a straight rod. A watch was placed in contact with one end of the straight rod, while the other was pressed gently against the tragus so as to shut the external meatus. The result was that the watch was heard nearly as distinctly as when in contact with the ear. When a similar experiment was performed with the angular portion of wood representing the chain of bones, the watch was also heard, but less distinctly than through the straight portion.

Experiment 2.—A tuning-fork C′, being made to vibrate, was placed in contact with one extremity of the angular piece of wood, the other being placed against the tragus of the ear, and as soon as the sound ceased to be heard, the straight portion was substituted, when the tuning-fork was again heard, and it continued to be heard for about three seconds.

Experiment 3.—A vibrating tuning-fork C′ was placed at one extremity of the angular piece of wood, the other extremity being held between the teeth, the fork was at first heard very distinctly, and when its sound could no longer be distinguished, the straight piece was substituted, and it was again heard for the space of two seconds.

Experiment 4.—Instead of the horizontal portion of wood representing the stapes, three portions of the same size were made into a triangle, and this was glued to the anterior surface of the inferior extremity of the piece representing the incus, thus . The previous experiment was then repeated with the substitution of this apparatus for the angular one, and with nearly the same result, viz. the fork was heard through the straight piece about three seconds after it had ceased to be heard by the apparatus representing the chain of bones

Experiment 5 —A piece of very thin paper was gummed over the end of a glass tube 6 inches in diameter, to the outer surface of this paper was glued a model of the chain of ossicles similar to the one used in the previous experiment, a vibrating tuning-fork C' being placed in the interior of the tube and within a quarter of an inch of the paper, the sound was heard through the free end of the chain placed between the teeth for ten seconds, when the sound ceased to be heard, a straight piece of wood was substituted, and the sound was not heard through it

II. *Experiments illustrative of the influence of the articulations in the chain*

Experiment 1 —Three pieces of wood, each about 5 inches long and half *an inch in thickness, were separated from each other* by pieces of india-rubber as thick as ordinary writing-paper, and they were then fastened together so as to assume the angular form possessed by the chain of ossicles. The tuning-fork C′ being placed at the free extremity of the chain, the other extremity being held between the teeth, it was found that the sound was heard as distinctly and for the same length of time, as when it passed through the chain formed of three portions glued together

Experiment 2 —When eight layers of the india-rubber were placed between each piece of wood, there was still very little difference in the intensity of the sound from that observed when it passed through the portions glued together

Experiment 3 —One, two, or three fingers having been placed between the first and second pieces of wood, and eight layers of india-rubber between the second and third, a very slight diminution in the intensity and duration of the sound was observed as compared with its passage through similar pieces when glued together

Experiment 4.—The back of the hand was placed in contact with the teeth, and the end of the vibrating fork C′ was pressed against the palm, the sound was heard very distinctly for several seconds; and when it ceased to be heard, a piece of solid wood 3 inches long was substituted, through which the sound of the fork was again heard faintly for four seconds

The inference from the two series of experiments above detailed is, that neither the variation of the plane existing in the chain of ossicles, nor the presence of the articulations, is sufficient to prevent the progress of sonorous undulations through this chain to the vestibule

The experiments and observations detailed above lead to the following conclusions —

1 That the commonly received opinion in favour of the sonorous undulations passing to the vestibule through the chain of ossicles is correct

2 That the stapes, when disconnected from the incus, can still conduct sonorous undulations to the vestibule from the air

3 *So far as our present experience extends*, it appears that in the human ear sound always travels to the labyrinth through two media, viz through the air in the tympanic cavity to the cochlea, and through one or more of the ossicles to the vestibule

VI "On the Electrical Discharge *in vacuo* with an Extended Series of the Voltaic Battery" By John P Gassiot, Esq, V P R S Received May 24, 1859

In a recent communication, since ordered for publication in the Philosophical Transactions, I described some experiments on the electrical discharge in a vacuum obtained by the absorption of carbonic acid with caustic potassa, and I showed that, when the discharge from an induction coil was passed through such a vacuum, the stratifications became altered in character and appearance as the potassa was more or less heated I have also in a former paper (Phil Trans 1858, p 1) shown that the stratified discharge can be obtained from the electrical machine

A description of an extended series of a water-battery was communicated by me as far back as December 1843 (Phil Trans 1844, p. 39). This battery consists of 3520 insulated cells some years had elapsed since it was last charged, and I found the zincs were very much oxidated, on again charging it with rain-water, I ascertained that there was sufficient tension to give a constant succession of minute sparks between two copper discs attached to the terminals of the battery, and placed about ⅛th of an inch apart. On attaching the terminals of the battery to the wires in a carbonic acid vacuum-tube inserted about 2 inches apart, I obtained a stratified discharge similar to that from an induction coil

The experiment was repeated with 400 series of Grove's nitric acid battery In this case distinct sparks between two copper discs were obtained, and the luminous layers were shown in a peculiar and striking manner, thus proving that the induction coil is not necessary for the production of the striæ, as in most of the experiments the only interruption of the battery circuit was through the vacuum-tube

I had another tube prepared, substituting for metallic points balls of gas-carbon At first the stratified discharge was obtained as before, while little or no chemical action took place in the battery , on heating the potassa, the character of the stratifications gradually changed, and suddenly a remarkably brilliant white discharge, *also stratified*, was observed , intense chemical action was at the same time perceptibly taking place in the battery, and on breaking the circuit, the usual vivid electrical flame-discharge was developed at the point of disruption

The continuation of these experiments will necessarily occupy much time, involving, as they do, the charging of so extended a series of the nitric acid battery, and with the requisite care necessary for the proper insulation of each cell , other phenomena were observed which require further verification, but I hope that after the recess the result which I hope to obtain may be of sufficient interest to form the subject of a future communication

VII "Note on the Transmission of Radiant Heat through Gaseous Bodies" By JOHN TYNDALL, Ph D , F R S &c Received May 26, 1859

Before the Royal Society terminates its present session, I am anxious to state the nature and some of the results of an investigation in which I am now engaged

With the exception of the celebrated memoir of M Pouillet on Solar Radiation through the atmosphere, nothing, so far as I am aware, has been published on the transmission of radiant heat through gaseous bodies. We know nothing of the effect even of air upon heat radiated from terrestrial sources

The law of inverse squares has been proved by Melloni to be true for radiant heat passing through air, whence that eminent experimenter inferred that the absorption of such heat by the atmosphere, in a distance of 18 or 20 feet, is totally inappreciable With regard to the action of other gases upon heat, we are not, so far as I am aware, possessed of a single experiment

Wishing to add to our knowledge in this important particular, I had a tube constructed, 4 feet long and 3 inches in diameter, and by means of brass terminations and suitable washers, I closed perfectly the ends of the tube by polished plates of rock-salt Near to one of its extremities, a T-piece is attached to the tube, one of whose branches can be screwed to the plate of an air-pump, so as to permit the tube to be exhausted, while the gas to be operated on is admitted through the other branch of the T-piece Such a tube can be made the channel of calorific rays of every quality, as the rock-salt transmits all such rays with the same facility

I first permitted the obscure heat emanating from a source placed at one end of the tube, to pass through the latter, and fall upon a thermo-electric pile placed at its other end The tube contained ordinary air When the needle of a galvanometer connected with the pile had come to rest, the tube was exhausted, but no change in the position of the needle was observed A similar negative result was obtained when hydrogen gas and a vacuum were compared

Here I saw, however, that when a copious radiation was employed, and the needle pointed to the high degrees of the galvanometer, to cause it to move through a sensible space, a comparatively large diminution of the current would be necessary, far larger, indeed, than the absorption of the air, if any, could produce while if I used a feeble source, and permitted the needle to point to the lower degrees of the galvanometer, the total quantity of heat in action was so small, that the fraction of it absorbed, if any, might well be insensible

My object then was to use powerful currents, and still keep the needle in a sensitive position, this was effected in the following manner —The galvanometer made use of possessed two wires coiled side by side round the needle; and the two extremities of each wire were connected with a separate thermo-electric pile, in such a manner that the currents excited by heat falling upon the faces of the two piles passed in opposite directions round the galvanometer A source of heat of considerable intensity was permitted to send its rays through the tube to the pile at its opposite extremity, the deflection of the needle was very energetic The second pile was now caused to approach the source of heat until its current exactly neutralized that of the other pile, and the needle descended to zero

Here then we had two powerful forces in perfect equilibrium, and inasmuch as the quantity of heat in action was very considerable, the absorption of a small fraction of it might be expected to produce a sensible effect upon the galvanometer-needle in its present position When the tube was exhausted, the balance between the equal forces was destroyed, and the current from the pile placed at the end of the tube predominated Hence the removal of the air had permitted a greater amount of heat to pass On readmitting the air, the needle again descended to zero, indicating that a portion of the radiant heat was intercepted Very large effects were thus obtained

I have applied the same mode of experiment to several gases and vapours, and have, in all cases, obtained abundant proof of calorific absorption Gases vary considerably in their absorptive power—probably as much as liquids and solids Some of them allow the heat to pass through them with comparative facility, while other gases bear the same relation to the latter that alum does to other diathermanous bodies

Different gases are thus shown to intercept radiant heat in different degrees I have made other experiments, which prove that the selfsame gas exercises a different action upon different qualities of radiant heat. The investigation of the subject referred to in this Note is now in progress, and I hope at some future day to lay a full description of it before the Royal Society

VIII. “Photochemical Researches”—Part IV. By Robert W. Bunsen, For Memb. R.S , and Henry Enfield Roscoe, Ph.D., Professor of Chemistry in Owens College, Manchester Received May 26, 1859

(Abstract)

In the three communications* which they have already made to the Royal Society upon the subject of photochemistry, the authors showed that they have constructed a most delicate and trustworthy instrument by which to measure the chemical action of light, and by help of which they have been able to investigate the laws regulating this action In the present memoir, the authors proceed, in the first place, to establish a general and absolute standard of comparison for the chemical action of light, and in the second place, to consider the quantitative relations of the chemical action effected by direct and diffuse sunlight They would endeavour, in this part of their research, to lay the foundation of a new and important branch of meteorological science, by investigating the laws which regulate the distribution, on the earth's surface, of the chemical activity emanating from the sun

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* Phil Trans 1857, pp 355, 381 and 601

The subject-matter of the present communication is divided under five heads —

1 The comparative and absolute measurement of the chemical rays

2 Chemical action of diffuse daylight

3 Chemical action of direct sunlight

4 Photochemical action of the sun, compared with that of a terrestrial source of light

5 Chemical action of the constituent parts of solar light

The first essential for the measurement of photochemical actions, is the possession of a source of constant light This the authors secured with a greater amount of accuracy than by the method described in their former communications, by employing a flame of pure carbonic oxide gas, burning from a platinum jet of 7 millims in diameter, and issuing at a given rate, and under a pressure very slightly different from that of the atmosphere The action which such a standard flame produces in a given time on the sensitive mixture of chlorine and hydrogen, placed at a given distance, is taken as the arbitrary unit of photochemical illumination This action is, however, not that which is directly observed on the scale of the instrument The true action is only obtained by taking account of the absorption and extinction which the light undergoes in passing through the various glass-, water-, and mica-screens placed between the flame and the sensitive gas These reductions can be made by help of the determinations given in Part III of these Researches, as well as by experiments detailed in the present Part When these sources of error are eliminated, it is possible, by means of this standard flame, to reduce the indications of different instruments to the same unit of luminous intensity, and thus to render them comparable For this purpose, the authors define the photometric unit for the chemically active rays, as the amount of action produced in one minute, by a standard flame placed at a distance of one metre from the normal mixture of chlorine and hydrogen, and they determine experimentally for each instrument the number of such units which correspond to one division on the scale of the instrument By multiplying the observed number of divisions by the number of photometric units equal to one division, the observations are reduced to a comparable standard It is proposed to call this unit a *chemical unit of light*, and ten thousand of them *one chemical degree of light*

According to this standard of measurement, the chemical illumination of a surface, that is, the amount of chemically active light which falls perpendicularly on the plane surface, can be obtained It has thus been found that the distance to which two flames of coal-gas and carbonic oxide, each fed with gas at the rate of 4 105 cubic cent per second, must be removed from a plane surface, in order to effect upon it an amount of chemical action represented by one degree of light, was, in the case of the coal-gas flame, 0 929 metre, in that of carbonic oxide 0 561 metre The chemical illuminating power, or chemical intensity, of various sources of light, measured by the chemical action effected by these sources at equal distances and in equal times, can also be expressed in terms of this unit of light, and these chemical intensities may be compared with the visible light-giving intensities In like manner, the authors define chemical brightness as the amount of light, measured photochemically, which falls perpendicularly from a luminous surface upon a physical point, divided by the apparent magnitude of the surface, and this chemical brightness of circles of zenith-sky of different sizes has been determined Experiment shows that the chemical brightness of various sized portions of zenith sky, not exceeding 0 00009 of the total heavens, is the same, or, that the chemical action effected is directly proportional to the apparent magnitude of the illuminating surface of zenith-sky

It is, however, important to express the photochemical actions not only according to an arbitrary standard, but in absolute measure, in units of time and space. This has been done by determining the absolute volume of hydrochloric acid formed by the action of a given source of light during a given space of time For this purpose, we require to know—

$v=$ the volume of hydrochloric acid formed by the unit of light
$h=$ the thickness of sensitive gas through which the light passed
$q=$ the surface-area of the insulated gas
$a=$ the coefficient of extinction of the chlorine and hydrogen for the light employed
$l=$ the number of observed units of light in the time t

When these values are known, the volume of hydrochloric acid which would be formed in the time t, by the rays falling perpendicularly on the unit of surface, if the light had been completely extinguished by passing through an infinitely extended atmosphere of dry chlorine and hydrogen, is found from the expression

$$V=\frac{v}{q}\,\frac{l}{1-10^{-ah}}$$

In this way the chemical illumination of any surface may be expressed by the height of the column of hydrochloric acid which the light falling upon that surface would produce, if it passed through an unlimited atmosphere of chlorine and hydrogen. This height, measured in metres, the authors propose to call a *Light-metre.* The chemical action of the solar rays can be expressed in light-metres, and the mean daily, or annual height thus obtained, dependent on latitude and longitude, regulates the chemical climate of a place, and points the way to relations for the chemical actions of the solar rays, which in the thermic actions are already represented by isothermals, isotherals, &c.

In order to determine the chemical action exerted by the whole diffuse daylight upon a given point on the earth's surface, the authors were obliged to have recourse to an indirect method of experimenting, owing to the impossibility of measuring the whole action directly, by means of the sensitive mixture of chlorine and hydrogen. For the purpose of obtaining the wished-for result, the chemical action proceeding from a portion of sky at the zenith, of known magnitude, was determined in absolute measure, and then, by means of a photometer, whose peculiar construction can only be understood by a long description, the relation between the *visible* illuminating power of the same portion of zenith sky and that of the total heavens was determined. As, in the case of lights from the same source but of different degrees of intensity, the *chemical* actions are proportional to the *visible* illuminating effects, it was only necessary, in order to obtain the chemical action produced by the total diffuse light, to multiply the chemical action of the zenith portion of sky by the number representing the relation between the visible illumination of the total sky and that of the same zenith portion

The laws according to which the chemical rays are dispersed by the atmosphere can only be ascertained from experiments made when the sky is perfectly cloudless In the determinations made with this specially-arranged photometer, care was therefore taken that the slightest trace of cloud or mist was absent, and the relation between the visible illuminating effect of a portion of sky at the zenith and that of the whole visible heavens, was determined for every half-hour from sunrise to sunset, the observations being made at the summit of a hill near Heidelberg, from which the horizon was perfectly free

The amount of chemical illumination which a point on the earth's surface receives from the whole heavens, depends on the height of the sun above the horizon and on the transparency of the atmosphere. If the atmospheric transparency undergoes much change when the sky is cloudless, a long series of experiments must be made before the true relations of atmospheric extinction of the chemical rays can be arrived at The authors believe, however, founding their opinion on the statement of Seidel in his classical research on the luminosity of the fixed stars, that the alterations in the air's transparency with a cloudless sky are very slight, and they therefore think themselves justified in considering the chemical illumination of the earth's surface, on cloudless days, to be represented simply as a function of the sun's zenith distance Although, from the comparatively small number of experiments which have been made, owing to the difficulty of securing perfectly cloudless weather, the constants contained in the formulæ cannot lay claim to any very great degree of accuracy, the authors believe that the numbers obtained are sufficient to enable them to determine the relation according to which the chemical energy proceeding from the sun is diffused over the earth when the sky is unclouded

From a series of observations made on June 6, 1858, the relation between the amount of light *optically* measured falling from the whole sky, and the amount (taken as unity) which, at the same time, falls from a portion of zenith sky equal to $\frac{1}{1000}$th of the whole visible heavens, has been calculated for every degree of sun's zenith distance from 20° to 90°, the results being tabulated, and also represented graphically These numbers, multiplied by the *chemical* light proceeding from the same portion of zenith sky for the same zenith distances, must give the chemical action effected by the whole diffuse daylight The amount of chemical light which falls from the zenith portion of sky is, however, the chemical brightness of that portion of sky. This chemical brightness has been determined, by the chlorine and hydrogen photometer, on various days, and at different hours, when the sky was perfectly cloudless A table contains the chemical action, expressed in degrees of light, which is effected on the earth's surface by a portion of zenith sky equal in area to $\frac{1}{1000}$th of the whole visible heavens, under the corresponding sun's zenith distances from 20° to 90° A curve representing the relation between the action and the height of the sun, shows that although the single observations were made on different years and at different times of the year and day, they all agree closely amongst themselves, and hence another proof is gained of the slight effect which variation in the air's transparency produces, and it is seen that the total chemical action effected by the diffuse light of day may be represented as a function of the sun's zenith distance

The numbers thus obtained have only to be multiplied by the corresponding numbers of the former table, in order to give the chemical action effected by the total diffuse light of day for zenith distances from 20° to 90° A table and graphic representation of these numbers is given. Knowing the relation between the sun's altitude and the chemical action, the chemical illumination effected each minute at any given locality at a given time may be calculated, this calculation has been made for a number of places for each hour on the vernal equinox, tables and curves representing the alteration of luminous intensity with the height of the sun at these places being given

From these data it is possible also to calculate the action produced by the whole diffuse light, not only for each minute, but during any given space of time For the following places the amount of chemical illumination expressed in degrees of light which falls from sunrise to sunset on the vernal equinox, is—

Melville Island	10590
Reykiavik	15020
St Petersburg	16410
Manchester	18220
Heidelberg	19100
Naples	20550
Cairo	21670

Experiment has shown that clouds exert the most powerful influence in reflecting the chemical rays, when the sky is partially covered by light white clouds, the chemical illumination is more than four times as intense as when the sky is clear Dark clouds and mists, on the other hand, absorb almost all the chemically active rays

The chemical action of the direct sunlight was determined by allowing a known fractional portion of the solar rays to fall perpendicularly on the insolation vessel of the chemical photometer. The solar rays reflected from the mirror of a Silbermann's heliostat were passed through a fine opening of known area into the dark room, and a large number of reductions and corrections had to be made in order to obtain, from the direct observations, the action, expressed in degrees of light, which the sun shining directly upon the apparatus would have produced if no disturbing influences had interfered. This action of direct sunshine was determined on three different cloudless days for various altitudes of the sun. As the sun approached the zenith the observed action rapidly increased, thus at 7^h 9′ A M, on September 15, 1858, when the sun's zenith distance was 76° 30′, the reduced action amounted to 5 5 degrees of light, whilst at 9^h 14′ A.M on the same day, the zenith distance being 58° 11′, the action reached 67 6 This increase in the sun's illuminating power is owing to the diminution in length of the column of air through which the rays pass If we suppose the atmosphere to be throughout of the density corresponding to a pressure 0·76 and a temperature 0°, and consider it as a horizontal layer, and if A represent the action effected before entrance into the atmosphere, the action, when the ray has passed through a thickness of atmosphere $=l$, is represented by

$$W_0=A10^{-\alpha l},$$

where $\frac{1}{\alpha}$ signifies the depth of atmosphere through which the ray has to pass to be reduced to $\frac{1}{10}$th of its original intensity, and where l is dependent on the atmosphere's perpendicular height $=h$, and the sun's zenith distance ϕ The numerical values of A α and l may be calculated from the direct observations, and hence the action W_1 effected at any other zenith distance ϕ_1, and under a pressure P_1, is found from the equation

$$W_1=A10^{\frac{-\alpha h P_1}{\cos\phi_1 P_0}},$$

where P_0 represents the atmospheric pressure under which A and α are calculated A comparison between the actions W_1 thus obtained, and those, W_0, found by experiment, shows as close an agreement as could be expected where the observational errors are necessarily so large.

From these experiments it is seen, that if the sun's rays were not weakened by passage through the atmosphere, they would produce an illumination represented by 318 degrees of light, or they would effect a combination in one minute on a surface on which they fell perpendicularly, of a column of hydrochloric acid 35 3 metres in height, assuming that the rays are extinguished by passing through an infinitely extended atmosphere of chlorine and hydrogen. By help of the above formula, it is also found that the sun's rays, after they have passed in a perpendicular direction through the atmosphere to the sea's level, under a mean pressure of 0 76 metre, only effect an action of 14 4 light-metres, or that under these conditions nearly two-thirds of their chemical activity have been lost by extinction and dispersion in the atmosphere. The total chemical action emanating from the sun during each minute is therefore represented by a column of hydrochloric acid 35 metres in height, and having an area equal to the surface of a sphere whose diameter is the mean distance of the earth to the sun. Or the light which the sun radiates into space during each minute of time represents a chemical energy, by means of which more than 25 billions of cubic miles of chlorine and hydrogen may be combined to form hydrochloric acid. In like manner the amounts of chemical action have been calculated, which the sun's rays, undiminished by atmospheric extinction, produce at the surface of the chief planets The first column of numbers gives the mean distances of the planets from the sun, the second contains the chemical action expressed in light-metres

Mercury	0 387	235 4	light-metres
Venus	0·723	67 5	,,
Earth	1 000	35 3	,,
Mars	1 524	15 2	,,
Jupiter	5 203	1 3	,,
Saturn	9 539	0 4	,,
Uranus	19 183	0 1	,,
Neptune	30·040	0 04	,,

By aid of the formula already given, the authors have been enabled to calculate the chemical action effected each minute by the direct sunlight, not only at different points on the earth's surface, but at various heights above the sea's level Both these series of relations are tabulated, and graphically represented On comparing the numbers and curves giving the action of the total diffuse light with those of the direct solar light, the singular fact becomes apparent, that from the North Pole to latitudes below that of Petersburg, the chemical action proceeding from the diffuse light is, throughout the day on the vernal equinox, greater than that effected by the direct sunlight, and that in lower latitudes, down to the Equator, the same phenomenon is observed, if not for the whole, still for a portion of the day It is further seen, that for all places, and on every day when the sun rises to a certain height above the horizon, there is a moment at which the chemical action of the diffused light is exactly equal to that of the direct sunlight The times at which these phases of equal chemical illumination occur can be calculated; they can also be actually determined, by allowing the direct sunlight alone, and the whole diffuse daylight alone, to fall at the same time upon two pieces of the same sensitised photographic paper, the period at which both papers become equally blackened, gives the time of the phase of equal chemical intensity Experiment proved not only that these points of equality which the theory requires actually occur, but also that the agreement between the calculated and observed times of occurrence of the phases is very close, giving proof that the data upon which the theory is founded are substantially correct

The formula, by help of which the chemical action of the direct sunlight effected at any place during any given time can be calculated, is next developed, and the direct solar action at the following places calculated for the vernal equinox from sunrise to sunset Column I gives the action of the direct sunlight during the whole day, expressed in degrees of light, Column II the action for the same time effected by both direct and diffuse solar light, and Column III the same action expressed in light-metres —

	I	II	III	
Melville Island	1196	11790	1306	metres
Reykiavik	5964	20980	2324	,,
St Petersburg	8927	25340	2806	,,
Manchester	14520	32740	3625	,,
Heidelberg	18240	37340	4136	,,
Naples . .	26640	47190	5226	,,
Cairo	36440	58110	6437	,,

The authors next proceed to examine the chemical brightness of the sun compared with a terrestrial source of light For this purpose the intensely bright light produced by a wire of magnesium burning in the air, was employed Experiment showed that the chemical intensity of the sunlight, undiminished by atmospheric extinction, is 128 times greater than that from a surface of incandescent magnesium of like apparent magnitude, or that burning magnesium effects the same chemical illumination as the sun when 9° 53′ above the horizon, supposing of course that both luminous sources present to the illuminated surface the same apparent magnitude. A totally different relation was found to exist between the visible illuminating power, i e. the effect produced on the eye, of the two sources in question. Thus, when the sun's zenith distance was 67° 22′, the chemical brightness of that source was 36 6 times, but the visible brightness 525 times as large as that of the terrestrial source of light

In the last section of this communication the chemical action of the constituent parts of the solar spectrum is investigated The sun's rays were reflected from a Silbermann's heliostat, and after passing through a narrow slit, they were decomposed by two quartz prisms The spectrum thus produced was allowed to fall upon a white screen covered with a solution of quinine, and any desired portion of the rays could be measured by a finely-divided scale, and the position noted by observation of the distances from the fixed lines For the purpose of identifying the fixed lines in the lavender rays, the authors were, by the kindness of Mr Stokes, allowed the use of an unpublished map of the most refrangible portion of the spectrum, prepared by that gentleman As the various components of white light are unequally absorbed by the atmosphere, it was obviously necessary to conduct all the measurements so quickly after one another, that no appreciable difference in the thickness of the column of air passed through should occur

This has been accomplished, and a series of exact measurements of the chemical actions of the spectrum for one particular zenith-distance of the sun obtained The action on the sensitive gas shows the existence of several maxima of chemical intensity in the spectrum Between the lines G in the indigo and H in the violet the greatest action was observed, whilst another maximum was found to lie near the line I in the ultra-violet rays. Towards the red or least refrangible end of the spectrum, the action became imperceptible about the line D in the orange, but at the other end of the spectrum the action was found to extend as far as Stokes's line U, or to a distance from the line H greater than the total length of the ordinary visible spectrum Tables and curves representing the action are given

IX. "On the Occurrence of Flint-implements, associated with the Remains of Extinct Mammalia, in Undisturbed Beds of a late Geological Period" By JOSEPH PRESTWICH, Esq, F R S, F G S &c Received May 26, 1859.

(Abstract)

The author commences by noticing how comparatively rare are the cases even of the alleged discovery of the remains of man or of his works in the various superficial drifts, notwithstanding the extent to which these deposits are worked, and of these few cases so many have been disproved, that man's non-existence on the earth until after the latest geological changes, and the extinction of the Mammoth, Tichorhine Rhinoceros, and other great mammals, had come to be considered almost in the light of an established fact Instances, however, have from time to time occurred to throw some doubt on this view, as the well-known cases of the human bones found by Dr Schmerling in a cavern near Liege,—the remains of man, instanced by M Marcel de Serres and others in several caverns in France,—the flint-implements in Kent's Cave,—and many more Some uncertainty, however, has always attached to cave-evidence, from the circumstance that man has often inhabited such places at a comparatively late period, and may have disturbed the original cave-deposit, or, after the period of his residence, the stalagmitic floor may have been broken up by natural causes, and the remains above and below it may have thus become mixed together, and afterwards sealed up by a second floor of stalagmite Such instances of an imbedded broken stalagmitic floor are in fact known to occur, at the same time the author does not pretend to say that this will explain all cases of intermixture in caves, but that it lessens the value of the evidence from such sources

The subject has, however, been latterly revived, and the evidence more carefully sifted by Dr Falconer, and his preliminary reports on the Brixham Cave*, presented last year to the Royal Society, announcing the carefully determined occurrence of worked flints mixed indiscriminately with the bones of the extinct Cave Bear and the Rhinoceros, attracted great and general attention amongst geologists This remarkable discovery, and a letter written to him by Dr Falconer on the occasion of his subsequent visit to Abbeville last autumn, instigated the author to turn his attention to other ground, which, from the interest of its later geological phenomena alone, as described by M Buteux in his "Esquisse Géologique du Departement de la Somme," he had long wished and intended to visit

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* On the 4th of May, this year, Dr Falconer further communicated to the Geological Society some similar facts, though singularly varied, recently discovered by him in the Maccagnone Cave near Palermo —See Proc Geol Soc

In 1849 M Boucher de Perthes, President of the "Société d'Émulation" of Abbeville, published the first volume of a work entitled "Antiquités Celtiques et Antédiluviennes," in which he announced the important discovery of worked flints in beds of undisturbed sand and gravel containing the remains of extinct mammalia Although treated from an antiquarian point of view, still the statement of the geological facts by this gentleman, with good sections by M Ravin, is perfectly clear and consistent Nevertheless, both in France and in England, his conclusions were generally considered erroneous, nor has he since obtained such verification of the phenomena as to cause so unexpected a fact to be accepted by men of science There have, however, been some few exceptions to the general incredulity. The late Dr Rigollot, of Amiens, urged by M Boucher de Perthes, not only satisfied himself of the truth of the fact, but corroborated it, in 1855, by his "Mémoire sur des Instruments en Silex trouvés à St Acheul" Some few geologists suggested further inquiry, whilst Dr Falconer, himself convinced by M de Perthes' explanations and specimens, warmly engaged Mr Prestwich to examine the sections

The author, who confesses that he undertook the inquiry full of doubt, went last Easter, first to Amiens, where he found, as described by Dr Rigollot, the gravel-beds of St Acheul capping a low chalk-hill a mile S.E of the city, about 100 feet above the level of the Somme, and not commanded by any higher ground The following is the succession of the beds in descending order.—

		Average thickness
1	Brown brick-earth (*many old tombs and some coins*), with an irregular bed of flint-gravel No organic remains	10 to 15 ft
	Divisional plane between 1 *and* 2*a very uneven and indented*	
2*a*	Whitish marl and sand with small chalk debris Land	

	Average thickness
and freshwater shells (*Lymnea*, *Succinea*, *Helix*, *Bithynia*, *Planorbis*, *Pupa*, *Pisidium*, and *Ancylus*, all of recent species) are common, and mammalian bones and teeth are occasionally found .	2 to 8 ft
2*b* Coarse subangular flint-gravel, – white with irregular ochreous and ferruginous seams,—with tertiary flint pebbles and small sandstone blocks Remains of shells as above, in patches of sand Teeth and bones of the elephant, and of a species of horse, ox, and deer,—generally near base This bed is further remarkable for containing worked flints ("Haches" of M de Perthes, and "Langues de Chat" of the workmen)	6 to 12 ft
Uneven surface of chalk	

The flint-implements are found in considerable numbers in 2*b* On his first visit, the author obtained several specimens from the workmen, but he was not successful in finding any himself On his arrival, however, at Abbeville, he received a message from M Pinsard of Amiens, to whose cooperation he expresses himself much indebted, to inform him that one had been discovered the following day, and was left *in situ* for his inspection On returning to the spot, this time with his friend Mr Evans, he satisfied himself that it was truly *in situ*, 17 feet from the surface, in undisturbed ground, and he had a photographic sketch of the section taken*.

Dr Rigollot also mentions the occurrence in the gravel of round pieces of hard chalk, pierced through with a hole, which he considers were used as beads. The author found several, and recognized in them a small fossil sponge, the *Coscinopora globularis*, D'Orb , from the chalk, but does not feel quite satisfied about their artificial dressing Some specimens do certainly appear as though the hole had been enlarged and completed

The only mammalian remains the author here obtained, were some specimens of the teeth of a horse, but whether recent or extinct, the specimens were too imperfect to determine , and part of the tooth of an elephant (*Elephas primigenius*?). In the gravel-pit of St. Roch, 1½ mile distant, and on a lower level, mammalian remains are far more abundant, and include *Elephas primigenius*, *Rhinoceros tichorhinus*, *Cervus somonensis*, *Bos priscus*, and *Equus**, but the workmen said that no worked flints were found there, although they are mentioned by Dr Rigollot.

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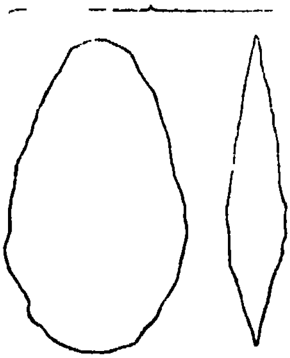
* On revisiting the pit, since the reading of this paper, in company with several geological friends, the author was fortunate to witness the discovery and extraction by one of them, Mr J W Flower, of a very perfect and fine specimen of flint-implement, in a seam of ochreous gravel, 20 feet beneath the surface They besides obtained thirty-six specimens from the workmen —June, 1859

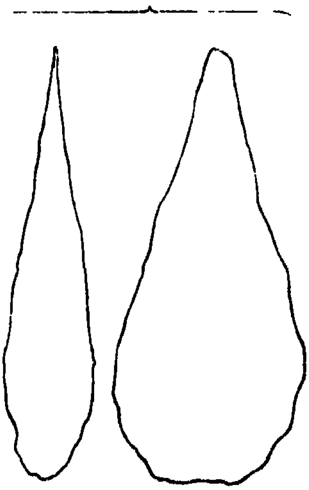
At Abbeville the author was much struck with the extent and beauty of M. Boucher de Perthes' collection. There were many forms of flints, in which he, however, failed to see traces of design or work, and which he should only consider as accidental, but with regard to those flint-instruments termed "axes" ("haches") by M. de Perthes, he entertains not the slightest doubt of their artificial make. They are of two forms, generally from 4 to 10 inches long: the outlines of two specimens are represented in the following diagram. They are very rudely made, without any ground surface, and were the work of a people probably unacquainted with the use of metals. These implements are much rarer at Abbeville than at Amiens, fig 1 being the common form at the former, and fig 2 at the latter place. The author was not fortunate enough to find any

Fig 1



Front section Side section

Fig 2



Side section Front section

One-third the natural size

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* To this list the author has to add the *Hyppopotamus*, of which creature four fine tusks were obtained on this last visit.

specimens himself, but from the experience of M de Perthes, and the evidence of the workmen, as well as from the condition of the specimens themselves, he is fully satisfied of the correctness of that gentleman's opinion, that they there also occur in beds of undisturbed sand and gravel.

At Moulin Quignon, and at St Gilles, to the S E of Abbeville, the deposit occurs, as at St Acheul, on the top of a low hill, and consists of a subangular, ochreous and ferruginous flint-gravel, with a few irregular seams of sand, 12 to 15 feet thick, reposing upon an uneven surface of chalk It contains no shells, and very few bones M de Perthes states that he has found fragments of the teeth of the elephant here The worked flints and the bones occur generally in the lower part of the gravel

In the bed of gravel also on which Abbeville stands, a number of flint-implements have been found, together with several teeth of the *Elephas primigenius,* and, at places, fragments of freshwater shells

The section, however, of greatest interest is that at Menchecourt, a suburb to the N W of Abbeville The deposit there is very distinct in its character, it occurs patched on the side of a chalk hill, which commands it to the northward, and it slopes down under the peat-beds of the valley of the Somme to the southward The deposit consists, in descending order, of—

		Average thickness
1	A mass of brown sandy clay, with angular fragments of flints and chalk rubble No organic remains Base very irregular and indented into bed No 2	2 to 12 ft
2	A light-coloured sandy clay ("sable gras" of the workmen), analogous to the loess, containing land shells, *Pupa*, *Helix*, *Clausilia* of recent species Flint axes and mammalian remains are said to occur occasionally in this bed	8 to 25 ft
3	White sand ("sable aigre"), with 1 to 2 feet of subangular flint-gravel at base This bed abounds in land and freshwater shells of recent species of the genera *Helix*, *Succinea*, *Cyclas*, *Pisidium*, *Valvata*, *Bithynia*, and *Planorbis*, together with the marine *Buccinum undatum*, *Cardium edule*, *Tellina solidula*, and *Purpura lapillus*. The author has also found the *Cyrena consobrina* and *Littorina rudis* With them are associated numerous mammalian remains, and, it is said, flint-implements	2 to 6 ft
4	Light coloured sandy marl, in places very hard, with *Helix*, *Zonites*, *Succinea*, and *Pupa* Not traversed	3 +

The Mammalian remains enumerated by M Buteux from this pit are,—*Elephas primigenius, Rhinoceros tichorhinus, Cervus somonensis?, Cervus tarandus priscus, Ursus spelæus, Hyæna spelæa, Bos primigenius, Equus adamaticus,* and a *Felis* It would be essential to determine how these fossils are distributed—which occur in bed No 2, and which in bed No 3 This has not hitherto been done The few marine shells occur mixed indiscriminately with the freshwater species, chiefly amongst the flints at the base of No 3 They are very friable and somewhat scarce It is on the top of this bed of flints that the greater number of bones are found, and also, it is said, the greater number of flint-implements The author, however, only saw some long flint flakes (considered by M de Perthes as flint knives) turned out of this bed in his presence, but the workmanship was not very clear or apparent, still it was as much so as in some of the so called flint knives from the peat beds and barrows There are specimens, however, of true implements ("haches") in M de Perthes' collection from Menchecourt, one noticed by the author was from a depth of 5, and another of 7 metres This would take them out from bed No 1, but would leave it uncertain whether they came from No 2 or No 3 From their general appearance, and traces of the matrix, the author would be disposed to place them in bed No 2, but M de Perthes believes them to be from No 3, if so, it must have been in some of the subordinate clay seams occasionally intercalated in the white sand

Besides the concurrent testimony of all the workmen at the different pits, which the author after careful examination saw no reason to doubt, the flint-implements ("haches") bear upon themselves internal evidence of the truth of M de Perthes' opinion It is a peculiarity of fractured chalk flints to become deeply and permanently stained and coloured, or to be left unchanged, according to the nature of the matrix in which they are imbedded In most clay beds they become outside of a bright opaque white or porcelainic, in white calcareous or siliceous sand their fractured black surfaces remain almost unchanged, whilst in beds of ochreous and ferruginous sands, the flints are stained of the light yellow and deep brown colours so well exhibited in the common ochreous gravel of the neighbourhood of London This change is the work of very long time, and of moisture before the opening out of the beds Now in looking over the large series of flint-implements in M de Perthes' collection, it cannot fail to strike the most casual observer that those from Menchecourt are almost always white and bright, whilst those from Moulin Quignon have a dull yellow and brown surface, and it may be noticed that whenever (as is often the case) any of the matrix adheres to the flint, it is invariably of the same nature, texture, and colour as that of the respective beds themselves In the same way at St Acheul, where there are beds of white and others of ochreous gravel, the flint-implements exhibit corresponding variations in colour and adhering matrix, added to which, as the white gravel contains chalk debris, there are portions of the gravel in which the flints are more or less coated with a film of deposited carbonate of lime, and so it is with the flint-implements which occur in those portions of the gravel Further, the surface of many specimens is covered with fine dendritic markings Some few implements also show, like the fractured flints, traces of wear, their sharp edges being blunted In fact, the flint-implements form just as much a constituent part of the gravel itself,—exhibiting the action of the same later influences and in the same force and degree,—as the rough mass of flint fragments with which they are associated

With regard to the geological age of these beds, the author refers them to those usually designated as post-pliocene, and notices their agreement with many beds of that age in England The Menchecourt deposit much resembles that of Fisherton near Salisbury, the gravel of St Acheul is like some on the Sussex coast, and that of Moulin Quignon resembles the gravel at East Croydon, Wandsworth Common, and many places near London The author even sees reason, from the general physical phenomena, to question whether the beds of St Acheul and Moulin Quignon may not possibly be of an age one stage older than those of Menchecourt and St. Roch, but before that point can be determined, a more extended knowledge of all the organic remains of the several deposits is indispensable

The author next proceeds to inquire into the causes which led to the rejection of this and the cases before mentioned, and shows that in the case of M. de Perthes' discovery, it was in a great degree the small size and indifferent execution of the figures and the introduction of many forms about which there might reasonably be a difference of opinion,—in the case of the arrow-heads in Kent's

Cave a hidden error was merely suspected,—and in the case of the Liege cavern he considers that the question was discussed on a false issue He therefore is of opinion that these and many similar cases require reconsideration, and that not only may some of these prove true, but that many others, kept back by doubt or supposed error, will be forthcoming

One very remarkable instance has already been brought under the author's notice by Mr Evans since their return from France In the 13th volume of the 'Archæologia,' published in 1800, is a paper by Mr John Frere, F R S and F S A, entitled "An Account of Flint-Weapons discovered at Hoxne in Suffolk," wherein that gentleman gives a section of a brick-pit in which numerous flint-implements had been found, at a depth of 11 feet, in a bed of gravel containing bones of some unknown animal, and concludes from the ground being undisturbed and above the valley, that the specimens must be of very great antiquity, and anterior to the last changes of the surface of the country,—a very remarkable announcement, hitherto overlooked

The author at once proceeded in search of this interesting locality, and found a section now exposed to consist of—

		feet
1	Earth and a few flints	2
2	Brown brick-earth, a carbonaceous seam in middle and one of gravel at base; no organic remains The workmen stated that two flint implements (one of which they shortly picked up in the author's presence) had been found about 10 feet from the surface during the last winter .	12
3	Grey clay, in places carbonaceous and in others sandy, with recent land and freshwater shells (*Planorbis*, *Valvata*, *Succinea*, *Pisidium*, *Helix*, and *Cyclas*) and bones of Mammalia	4
4	Small subangular flint-gravel and chalk pebbles	2½
5	Carbonaceous clay (stopped by water)	½+

The weapons referred to by Mr Frere are described by him as being found abundantly in bed No. 4, but at the spot where the work has now arrived, this bed is much thinner, and is not worked In the small trench which the author caused to be dug, he found no remains either of weapons or of bones He saw, however, in the collection of Mr. T. E Amyot, of Diss, specimens of the weapons, also an astragalus of the elephant from, it was supposed, this bed, and, from bed No 3, the teeth of a horse, closely resembling those from the elephant-bed of Brighton

The specimens of the weapons figured by Mr Frere, and those now in the British Museum and elsewhere, present a singular similarity in work and shape to the more pointed forms from St Acheul

One very important fact connected with this section, is that it shows the relative age of the bone and implement-bearing beds They form a thin lacustrine deposit, which seems to be superimposed on the Boulder Clay, and to pass under a bed of the ochreous sand and flint-gravel belonging to the great and latest drift-beds of the district

The author purposely abstains for the present from all theoretical considerations, confining himself to the corroboration of the facts —

1 That the flint-implements are the work of man.

2 That they were found in undisturbed ground

3 That they are associated with the remains of extinct Mammalia

4 That the period was a late geological one, and anterior to the surface assuming its present outline, so far as some of its minor features are concerned

He does not, however, consider that the facts, as they at present stand, of necessity carry back Man in past time more than they bring forward the great extinct Mammals towards our own time, the evidence having reference only to relative and not to absolute time, and he is of opinion that many of the later geological changes may have been sudden or of shorter duration than generally considered In fact, from the evidence here exhibited, and from all that he knows regarding drift phenomena generally, the author sees no reason against the conclusion that this period of Man and the extinct Mammals—supposing their contemporaneity to be proved—was brought to a sudden end by a temporary inundation of the land, on the contrary, he sees much to support such a view on purely geological considerations

The paper concludes with a letter from Mr. John Evans, F.S A and F.G S, regarding these implements from an antiquarian rather than a geological point of view, and dividing them into three classes:—

1. Flint flakes,—arrow-heads or knives

2 Pointed weapons truncated at one end, and probably lance or spear heads (fig. 2)

3 Oval or almond-shaped implements with a cutting edge all round, possibly used as sling-stones or as axes (fig 1)

Mr Evans points out, that in form and workmanship those of the two last classes differed essentially from the implements of the so-called Celtic period, which are usually more or less ground and polished, and cut at the wide and not the narrow end, and that had they been found under any circumstances, they must have been regarded as the work of some other race than the Celts, or known aboriginal tribes He fully concurs with Mr Prestwich, that the beds of drift in which they were found were entirely undisturbed

X “Observations on the Discovery in various Localities of the Remains of Human Art mixed with the Bones of Extinct Races of Animals” By CHARLES BABBAGE, Esq, M A., F R.S &c Received May 26, 1859

Statements have recently been made relative to the discovery of works of human art occurring in a breccia amongst bones of ancient animals, hitherto supposed to have been extinct long anterior to the existence of our race These observations are supposed by some to prove the great antiquity of the human race, whilst others, equally competent to form an opinion, admit that the intermixture of such remains presents a most perplexing mystery

Whatever may be the result of yet unpublished or of future and more extensive observations, it is certainly premature to assign this great antiquity to our race, as long as the occurrence of such mixtures can be explained by known causes admitted to be still in action

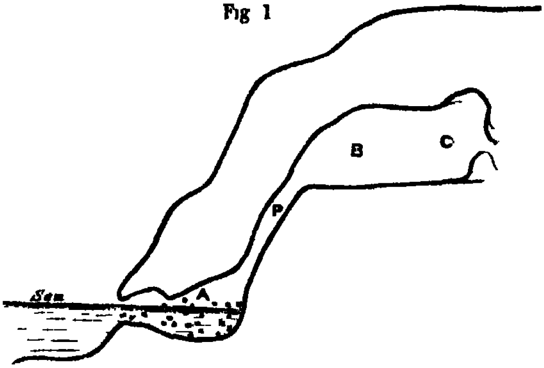
Two places have recently been pointed out in which such mixtures are stated to occur —1st, certain localities in France, 2nd, certain caves in Sicily The latter have been visited by Dr Falconer, and as the information respecting them which we at present possess, though small, is yet much more definite than what is known of the French locality, my explanations will chiefly relate to the *latter*

It is stated that one of the Sicilian caves has its sides perforated by marine animals

That on penetrating the stalactitical incrustation covering the *roof* of the cavern, and detaching fragments, it was found to consist of a breccia of bones of animals long extinct, mixed with fragments of flint or stone, bearing evident traces of human art

In order to explain these circumstances, it is only necessary to admit the upheaval of the land and the occurrence of torrents

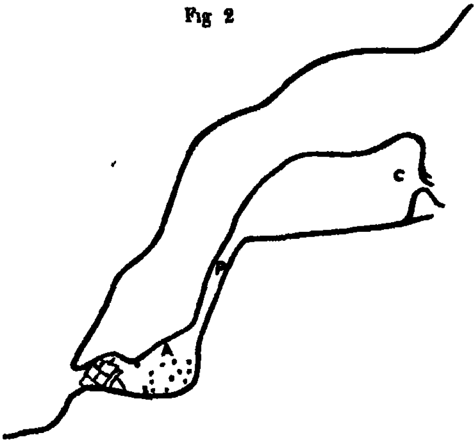
Fig 1



1*st Period* —Let us suppose two caverns, a lower, A, and an upper, B C, communicating with each other by a long rent or pipe P This pipe may be supposed of any height, sufficient when filled with water to produce the required force

The lower cavern is supposed to be nearly at the level of the sea

Fig 2



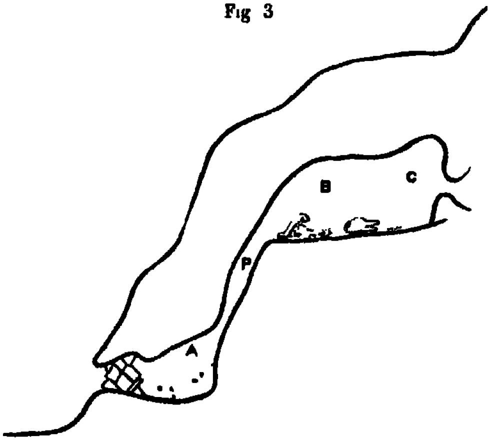
Pholades and other marine animals will perforate or attach themselves to the bottom and sides of the cavern, and if the sea entirely fill it the roof, too, or at least that portion adjacent to the mouth of the cavern, may be similarly affected The rocks in which these caverns occur may be of any geological age.

2nd Period—By the gradual or quick upheaval of the strata in which these caverns occur, they may become dry.

During the rising, or at a later period, fragments of rock may have accumulated at the open mouth of the lower cavern, and thus have stopped up its entrance, leaving the roof, sides, and floors bearing evident traces of having been an ocean cave

3rd Period—Ages may have elapsed during which other strata may have been depositing in other portions of our globe But ultimately the earth became inhabited by those ancient, but now extinct mammalia, whose remains abound in its caverns

Fig 3

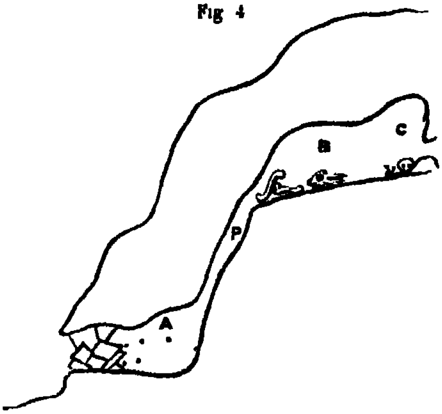


4th Period—Ages may again have intervened when man, in his first rude state of existence, entering this deserted den through the opening its former occupants had used, might have been glad to shelter himself from an inclement climate in this bone-house of a more ancient world.

During this period traces of man's skill would probably be left within his miserable abode, pottery, if he possessed the art, charcoal or charred wood, if he were acquainted with fire, rude cutting instruments of flint or other hard stone, perhaps spear or arrow-heads

Fig 4 represents the caverns at the end of the fourth period

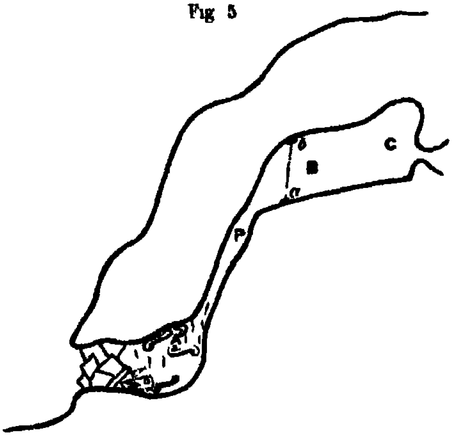
Fig 4



5th Period —Torrents entering by the aperture at C, might now have swept through the upper cavern, perhaps at successive intervals of time The effect of such torrents would be to wash from the sloping floor of the upper cavern, the mixed remains of animal and human life, together with the earliest traces of human art

In some of these catastrophes, the workman, as well as his work, may have been entombed together in the lower cavern

Fig 5



Thus in the course of time the whole of the lower cavern may have been filled up even to the roof

This state is represented in fig 5

The effect of infiltration through the roof would now cement into a breccia the mingled remnants from the upper chamber, and enclose them, as it were, in a marble monument, in their new, though not their latest resting-place

But the infiltration proceeding from the roof would act first, and most powerfully, in cementing the upper part of the intruded mass

The lower portion would be less consolidated, and therefore less capable of resisting any pressure from above

6th Period —In the progress of time, some torrent of extraordinary magnitude may have penetrated through the upper cavern, and, filling the channel or pipe P, have acted by its hydrostatic pressure on the semi-consolidated matter in the lower one As the water accumulated in the pipe, the pressure would become immense, and ultimately the materials included in the lower cavern would give way at their point of least resistance This would probably be towards the middle or bottom of the original entrance The first sudden rush would probably clear out the greater part of the contents of the lower cavern, leaving, however,—

1st Those portions attached to the roof by the infiltrated matter

2nd Those portions on the floor more consolidated by pressure than the middle which gave way, and also other portions of the loose rubbish which the form of the floor at the entrance of the cave might have intercepted in its course

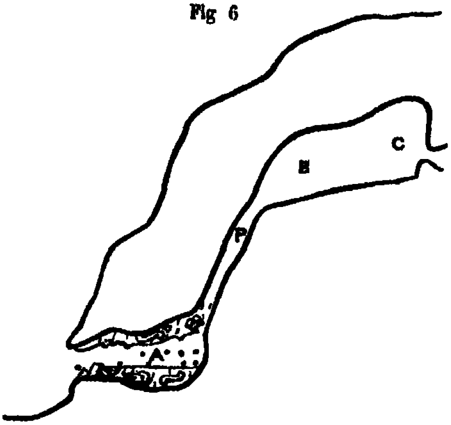
After this reopening of the lower cavern, the access of air would accelerate evaporation from its roof, and that portion of the breccia still adhering to it would gradually acquire the external coating of stalactite which usually occurs in caves

In the meantime the dropping from the roof, falling upon the floor, might also contribute to consolidate the remaining fragments, and, although more slowly, cover the whole of them with a stalagmitic floor

In this state, on entering the lower cave, the walls would be found perforated by marine animals, and no traces of animal life or of human art would present itself, but on excavating the floor, both would be found below the stalagmite, whilst if the curious inquirer should drive his pick into the roof, its fragments would bear testimony to the same fact

Fig 6 represents the final state of the cavern

Fig 6



That caverns are occasionally filled with water, and after remaining full perhaps for centuries, are drained by artificial or natural causes, is well known A very interesting case presented itself to me when visiting the caverns of Mitchelstown in Ireland

These caverns had recently (1833) become accessible, and were then very imperfectly explored. I expressed to the guides my wish to visit some of the unexplored portion, and, after traversing various chambers during six hours, we entered a long and lofty cavern, the floor of which sloped rather steeply towards one side. The whole floor was covered with a coat of soft red mud, about three inches thick, still holding a portion of the water in which it had been suspended. No trace whatever of the footsteps of man or of animals appeared, the impression of our own feet alone marked the track up to the point which we had reached Being rather in advance of my companions, my attention was suddenly attracted by what appeared to me to be about a bushel of soot lying in a small heap on the floor. On examination, I found it to consist of a moist spongy substance, of a black colour, which might, if dry, have assumed the form of a coarse black powder. Asking the opinion of my guide, he suggested that it might have been the remains of a fire lighted by some previous explorer; but this was inadmissible I looked round for matter of the same kind, but on further search I could not detect any other instance, however, accidentally casting my eye towards the roof of the cavern, I observed a black patch vertically above the heap of supposed soot lying on the floor, and from 20 to 30 feet above it The dotted line, *a* *b*, fig 5, may represent the position

On my return from the cavern I examined the black sooty matter, and found that it left but very small traces under the action of the blowpipe

On the following day, having made inquiries as to the drainage of the neighbouring country, I was informed that about twenty years before my visit, a stream of water had been diverted from the valley in which it originally flowed, into another adjacent valley

I then visited several quarries, in one of which, about a mile from the caves, I observed a small stream of water terminating in a little pool or sink In this pool I noticed slight eddies, which occasionally sucked in very small particles floating on the surface of the water

I now visited the valley from which the original stream had been diverted, and found at some elevation a peat bog to which it had probably given rise This peat was in several parts nearly black, the most decayed portion greatly resembled the black matter I had brought from the cavern The origin of this black matter in the cavern now became apparent

The large caverns I had visited were considerably below the level of the peat moss, and the stream which flowed through it A portion of its waters was conveyed by sinks and crevices into the caves, and kept them continually full There must, however, have been some very small leakage through which, when the stream which supplied the water was cut off, those caverns were, after many years, laid dry.

A small mass of unconsolidated peat, sufficiently light to float, must have been conveyed by the water into those caverns When it arrived at the spot on which the black matter was found, the piece of peat which still floated must have been pressed against the roof of the cavern Remaining there undisturbed for years, it may have become by decomposition specifically heavier than water, and then have subsided vertically down on the floor to the place on which I found it, leaving in the black spot on the roof the certificate of its former residence. On the other hand, the piece of peat may have retained its power of floating, and only have descended to the floor of the cavern by the slow escape of the water

Such circumstances as these ought to induce us to examine with great caution, any instances of the occurrence of works of human art mixed with the remains of animals not yet proved to have been co-existent with man Accident might have conveyed and hidden in the Mitchelstown caverns, a portion of human dress instead of a patch of peat It is obvious that under slightly altered circumstances, instruments formed by man, the bony remains of his frame, or those of other recent animals, might, by still existing causes, be conveyed into deep recesses in the bowels of the earth, and there deposited with the remains of animals of an entirely different geological age

Cases might occur in which the water passing in larger quantity would convey into such caverns a quantity of suspended mud, differing in its character at various seasons, and thus silt up the *confused relics of distant ages in a regularly stratified deposit*

It is quite possible that the human remains might thus be enclosed beneath the stalagmitic crust, whilst the more ancient remains were scattered above it, uncovered, or covered by another coat of stalagmite

If we suppose the existence of two upper caves, B and C, at different heights, each separately communicating with the lowest cave, A, as in fig 7, then still more remarkable facts might ultimately present themselves to those whom accident should lead to examine the lowest cave If, for example, the highest of these caves (C) contained only the remains of the extinct races of animals, and the other, or middle cave (B), nothing but those of man and the works of his own hands, the following series of events might occur —

1st A flood directing its course wholly through the middle cave (B), might wash down the fragments of the bones and works of man from that cave, and deposit them on the floor of the lowest cavern (A)

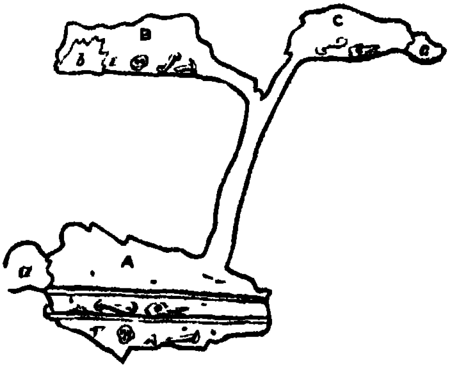
2nd In a long series of years, a thick stalagmitic covering might be formed, giving an entire new stony floor to that cavern

3rd Afterwards another flood, rising to a higher level, accidentally taking its course through the highest cave (C), might cover this new stalagmitic floor of the lowest cave (A) with the bony fragments of these more ancient animals

4th The continued infiltration from above might again cover these remains with another thick coating of stalagmite, such alterations might even be several times repeated

The annexed sketch will explain this case more clearly —

Fig 7



The great geological law, that the order of succession of strata indicates the order of their antiquity, the lowest being always the oldest, is limited by the condition that those strata shall not have been removed from their original beds

But the action of causes still existing may have produced apparent deviations from this order, and the present state of geological science seems to require an examination of such exceptional cases

If a great river, similar to the Mississippi or the Amazon, flowed through a country whose superficial stratum consisted of a thin bed of chalk succeeded by gault, then during thousands of ages it would distribute, by means of an ocean current, its milky burden over the bottom of the existing ocean, on which, after a lengthened interval, a bed of chalk would reappear

When the superficial bed of chalk over which the river flowed was cut through, its waters would begin to act on the newly-exposed gault, and after another period of equally vast length, a stratum of gault might cover the chalk, thus producing an extensive inversion of the two strata

But this would be dependent on the relative fineness and specific gravity of the particles of the two substances It is *possible* that the particles of gault, if larger or of greater specific gravity than those of the chalk, might arrive at a greater terminal velocity, and even overtake and pass through those of the chalk still suspended, thus forming a bed below the chalk, or a mixed bed of chalk passing into gault, and then ultimately a bed of gault above the chalk

In such circumstances few or none of the larger fossils would occur, but possibly the remains of infusorial animals might enable us to identify the material of the ancient and of the new but inverted deposit of gault

The case of remains of human art found imbedded with bones of extinct races of animals in deposits of ancient gravel, seems to require a different explanation

Admitting, however, the existence of those animals to have been contemporaneous with the original distribution of the gravel, it by no means certainly follows that the race of man was coeval with them

For the remains of man and his rude arts might occur on the surface of that gravel long ages after the extinction of those races of animals Several causes might produce their mixture —

1st A vast lake bursting its barriers by erosion, or by an earthquake, might carry before it in its impetuous course the superficial remains of man, mixed up with vast quantities of gravel, containing the bones of the extinct races of animals, and deposit them over a large area of land at a lower level

2nd The change of the course of a river, or of a branch of its delta, might produce the same mixture of the remains of two distinct and far distant ages It might, by the clearing out of its new channel, carry off the gravel and the remains of extinct animals, and deposit them, mixed up with specimens of human art, on spots which, after a few centuries, might again reappear as dry land

3rd A narrow pass, the outlet of a stream of water, might be stopped up by the avalanches falling from a glacier after a severe winter, and the lake formed by the stream might thus periodically rise, until the pressure broke through the barrier

4th. Amongst the phenomena occurring during earthquakes, it has been observed that large cracks have suddenly opened and as suddenly closed, either immediately or shortly after During these momentary or temporary openings, the remains of the arts of man, and even man himself, may have dropped into the chasm Under such circumstances, remains of man and his arts might occur in formations of any date If the cleft occurred in rock or in any very hard material, traces of it would remain to indicate their origin If it occurred in clay or softer material, the track through which these remains entered might be partially or even entirely obliterated If the cleft occurred in tolerably compact gravel and then immediately closed, it would scarcely be possible at a future period to trace its origin

The discussion of the recent observations of Mr Prestwich on flints, worked apparently by the hand of man, found deeply imbedded in ancient gravel, as well as the extensive observations of Dr Falconer on the bone-caves of Sicily, have given a new and important interest to the great question of the antiquity of the residence of our race on the planet we inhabit

Having examined a few of these flint-instruments, I am satisfied that several of them have been worked by human hands This opinion is founded upon the previous examination many years ago of the mode then used for making gun-flints

Amongst the many valuable observations of Dr Falconer, one fact to which he testifies deserves the most marked attention, and may possibly assist in directing us to the true solution of the problem

Dr Falconer has noticed the fact that the greater portion of these bones belong to the Hippopotamus, and also that they occur in their several deposits in enormous numbers In each cave there must have been several thousands, if not tens of thousands, of individuals The question immediately suggests itself, what causes produced this vast collection of individuals of the same race entombed in one common sepulchre?

It is scarcely possible to suppose that any instinct could have led the Hippopotamus, when death approached, to have chosen particular spots where the bones of his race were exposed to his view If this were so, then most probably the existing race would possess the same instinct

Another question arises Were these remains originally deposited in different localities, and afterwards transported by some common cause to these various caverns and beaches? Water seems the only probable mode of conveyance if this were so, traces of the rolling action of water must be found on *all* the bones, but this I apprehend is not the case. Moreover, it is difficult to conceive how water could have collected the bones of a multitude of individuals of the *same* race, distributed over a wide extent of country, into a few favoured localities The bones of all other animals inhabiting the same country, and remaining on its surface, would have been exposed to the same action, and should have been deposited in the same tomb If these animals all perished at the same time in each locality, some common cause must have produced the catastrophe

Although the existing evidence may be insufficient to lead us to the true solution of this interesting question, yet it may be useful to throw out hypotheses, which, by accounting for some of the facts, shall direct the attention of future observers to the examination of such special points as may either partially support or directly disprove these conjectures

With this view I shall offer two conjectures, one dependent on the subsidence of the land, the other upon the rising of the waters

Conjecture I —By the subsidence of the land

Let us imagine that the basin of the bottom of the Mediterranean had at a former period been on a level with, or just above the African continent Sicily and the various islands would then have stood above it as mountain chains

One portion of the drainage of this land may have been effected by a vast river passing into the Atlantic through the opening now known as the Straits of Gibraltar

In this state of things extensive freshwater lakes and other large rivers may have contributed to support large herds of Hippopotami

In such circumstances the gradual subsidence of this land would check the outflow of its rivers, and occasion extensive marshes and lakes, a state of things favourable to the increase of the Hippopotami As the subsidence proceeded, those animals which dwelt on the banks of the lakes and rivers would be driven inland. The waters of the Atlantic, entering through the channel of the former river, would convert the low ground and the marshes into sea, and thus gradually isolate the sinking land from the African coast

When this state of the country had caused the Hippopotami to multiply rapidly, an increased rapidity in the sinking of its lands through the waters, might drive those animals rapidly to the higher lands, which would then form islands, on the borders of which they would congregate in multitudes, until numbers of them, collected in ravines, rushing on in their attempts to escape in search of food, might trample each other to death and leave their bodies at the foot of impassable precipices washed by this new sea

Conjecture II —Let us suppose that the basin of the Mediterranean was formerly dry land shut in from the Atlantic by a barrier at the Straits of Gibraltar In these circumstances the whole of the present Mediterranean Sea would have been a country sunk more or less under the level of the ocean, just as a large tract of country around the Dead Sea is at present

The country included in this great depression might be warmer than others situated in the same latitude, and would be full of lakes fed by rivers, since there could be no drainage except from evaporation It would therefore probably be favourable to the growth of the Hippopotamus

Imagine now some convulsion to have opened the Straits of Gibraltar, so as to have allowed the waters of the Atlantic to enter this vast basin The salt river thus introduced might require days, or weeks, or months, or years, before it filled this immense cavity, it might also increase its velocity as it wore away the channel of its entrance.

Under these circumstances the Hippopotami would be gradually driven to the higher ground, and those mountainous regions which now form the islands of the Mediterranean would then have received on their shores the hosts of animals, driven by this inundation to seek dry land on which to repose and food on which they might subsist

Both of these hypotheses account for the aggregation in different localities, of the remains of large numbers of animals of the same class, dwelling amongst rivers and lakes, both equally account for their destruction on the same spots, either by trampling each other to death in the rush to escape, or by the slower processes of drowning or starvation But neither hypothesis accounts for the fractured state of these bones, even though the animals should have rushed *over* the precipice It might be expected that some portion of these Hippopotami, escaping from the deluge which destroyed their race, would have reached the plains of some larger island, and then separating in search of food, have left their carcases, wasted by famine, variously scattered at a distance from each other

The following questions are thus suggested for inquiry —

Are the bones of young and of old animals mixed indiscriminately throughout the whole of these bone-beds?

Is it possible to distinguish the bones of females from those of males?

Do the bones of the larger and stronger individuals occur in greater abundance near the top of the bone-beds, and those of the smaller and feebler animals nearer the bottom of those beds?

If the sea followed these animals quickly, the young and the feeblest would perish before they reached the great deposits of bones

Although not at all confident that either of these explanations is the true one, I look upon them as open to less objection than any other of which I have yet heard, and therefore give them a temporary assent

The conclusion to which these remarks lead, is that whilst we ought to be quite prepared to examine any evidence which tends to prove the great antiquity of our race, yet that if the facts adduced can be explained and accounted for by the operation of a few simple and natural causes, it is unphilosophical to infer the coexistence of man with those races of extinct animals

The interest and importance of the subject are such, that new and still more extensive researches cannot fail to be made, and if these remarks shall in any way contribute to lighten the labour of future inquirers, or to promote the true explanation of the facts, they will have fully attained the object of their publication

XI "Remarks on Colour-Blindness" By Sir JOHN F W. HERSCHEL, Bart, F R S.

[Extracted from a Report by Sir J F W H on Mr. Pole's paper on the same subject*, and communicated at the request of the President and Council]

I consider this paper as in many respects an exceedingly valuable contribution to our knowledge of the curious subject of colour-blindness—1st, because it is the only clear and consecutive account of that affection which has yet been given by a party affected, in possession of a knowledge of what has yet been said and written on it by others, and of the theories advanced to account for it, and who, from general education and habits of mind, is in a position to discuss his own case scientifically, and 2ndly, for the reasons the author himself alleges why such a person is really more favourably situated for describing the phenomena of colour-blindness, than any normal-eyed person can possibly be It is obvious that on the very same principle that the latter considers himself entitled to refer all his perceptions of colour to three primary or elementary sensations—whether these three be red, blue, and yellow, as Mayer (followed in this respect by the generality of those who have written on colours) has done, or red, green, and violet, as suggested by Dr Young, reasoning on Wollaston's account of the appearance of the spectrum to his eyes—on the very same principle is a person in Mr Pole's condition, or one of any other description of abnormal colour-vision, quite equally entitled to be heard, when he declares that he refers his sensations of colour to two primary elements, whose combination in various proportions he recognizes, or thinks he recognizes, in all hues presented to him, and which, if he pleases to call yellow and blue, no one can gainsay him, though, whether these terms express to him the same sensations they suggest to us, or whether his sensation of light with absence of colour corresponds to our white, is a question which must for ever remain open (although I think it probable that such is really the case). All we are entitled to require on receiving such testimony is, that the party giving it should have undergone that sort of *education of the sight and judgment*, especially with reference to the prismatic *decomposition* of natural and artificial colours, for want of which the generality of persons whose vision is unimpeachably normal, appear to entertain very confused notions, and are quite incapable of discussing the subject of colour in a manner satisfactory to the photologist

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* "Proceedings," vol viii p 172, and vol. ix p 716

It is as necessary to distinguish between our sensations of colour, and the qualities of the light producing them, as it is to distinguish between bitterness, sweetness, sourness, saltness, &c, and the chemical constitution of the several bodies which we call bitter, sweet, &c. Whatever their views of prismatic analysis or composition might suggest to Wollaston and Young, I cannot persuade myself that either of them recognized the *sensation* of greenness as a constituent of the sensations they received in viewing chrome yellow, or the petal of a Marigold on the one hand, and ultramarine, or the blue Salvia on the other, or that they could fail to recognize a certain redness in the colour of the violet, which Newton appears to have had in view when he regarded the spectrum as a sort of octave of colour, tracing in the repetition of redness in the extreme refrangible ray, the commencement of a higher octave too feeble to affect the sight in its superior tones Speaking of my own sensations, I should say that in fresh grass, or the laurel leaf, I do not recognize the sensation either of blue or of yellow, but something *sui generis*, while, on the other hand, I never fail to be sensible of the presence of the red element in either violet, or any of the hues to which the name of purple is indiscriminately given, and my impression in this respect is borne out by the similar testimony of persons, good judges of colour, whom I have questioned on the subject

I would wish, then, to be understood as bearing in mind this distinction when speaking of the composition of colours by the superposition of coloured lights on the retina It seems impossible to reason on the joint or compound sensation which ought to result from the supraposition in the sensorium of any two or more sensations which we may please to call primary, so that if, following common usage, I speak in what follows of red, yellow, and blue (or in reference to Young's theory of red, green, and violet) as *primary colours*, I refer only to the possibility of producing all coloured sensations by the union on the retina of different proportions of lights, competent separately to produce those colours, which is purely a matter of experience

It is necessary to premise this, when I remark that I by no means regard as a logical sequence Mr. Pole's conclusion in § 15, that because he perceives as colours only yellow and blue, *therefore* the neutral impression resulting from their union must be that sensation which the normal-eyed call green On the contrary, I am strongly disposed to believe that he sees white as we do, for reasons which I am about to adduce

Mr Maxwell has lately announced his inability to form green by the combination of blue and yellow On the other hand, the prismatic analysis of the fullest and most vivid yellows (those which excite the sensation of yellowness in the greatest perfection), as the colours of bright yellow flowers, or that of the yellow chromate of mercury, clearly demonstrates the fullness, richness, and brilliancy of their colour to arise from their reflexion of the whole, or nearly the whole of the red, orange, yellow, and green rays, and the suppression of all, or nearly all the blue, indigo, and violet portion of the spectrum. On the hypothesis of an analysis of sensation corresponding to an analysis of coloured light, these facts would seem incompatible with the simplicity of the sensation yellow, and it would appear impossible (on that hypothesis) to express them otherwise than by declaring red and green to be primary sensations, and yellow a mixture of them—a proposition which needs only to be understood to be repudiated—quite as decidedly as that the *sensation* of greenness is a mixture of the *sensations* of blueness and yellowness, and for the same reason, the complete want of suggestion of the so-called simple sensations by the asserted complex ones

Mr Maxwell's assertion that blue and yellow do not make green, assuredly appears startling as contradictory to all common experience, but the common experience appealed to is that of artists, dyers, and others in the habit of mixing natural colours as they are presented to us in pigments, coloured tissues, &c, who have for the most part never seen a prismatic spectrum, or at least attended to its phænomena The perceptions of colour afforded by such objects are those of white light from which certain rays have been abstracted by absorption, that is to say, they are *negative hues*, or hues of darkness rather than of light, inasmuch as all the colouring of the artist is based, not on the generation, but on the destruction of light This circumstance, which is not generally recognized, even among educated artists, has vitiated all the language of chromatics as applied to art, and so placed a barrier between the painter and the photologist, which has to be surmounted before they can come to a right understanding of each other's meaning It is evident, that, to make experiments on the subject free from this objection, absorptive colours must be discarded, at least in bodily mixture with each other Thus it is true that a dingy green may be produced by rubbing together in powder prussian blue and the yellow chromate of mercury above mentioned, but both these agree in reflecting a considerable, and the latter a very large proportion of green light, to the predominance of which in the joint reflected beam its tint is owing So also, when blue and yellow liquids (not acting chemically on each other) are mixed, as in water colour drawings, greens, sometimes very lively ones, are produced In these cases the yellow absorbs almost all the whole of the incident blue, indigo, and violet light, and the blue a very large proportion of the red, orange, and yellow, both allowing much green to pass, and to *this*, rather than to a mixture of the other rays, the resulting tint is due

In the light transmitted by cuprate of ammonia of a certain thickness, the red, orange, yellow, and green are wholly extinguished, while the blue, indigo, and violet are allowed to pass The result is the fullest and *bluest* blue it is possible to obtain From this result, compared with that derived from the analysis of natural yellows, it follows that the union on the retina of the yellowest yellow, and the bluest blue, in such proportions that neither shall be in excess, so as to tinge the resulting light either yellow or blue, is *not green, but white* The same conclusion follows from dividing the spectrum into two, the one portion containing all the less retrangible rays up to the limit of the green and blue, the other all the remaining rays If the blue portion be suppressed, and the remainder reunited by a refraction in the opposite direction, the resulting beam is *yellow*, if the other, *blue*, both vivid colours—but if neither, *white* of course, and not green, results from the exact recomposition of the original white beam

It may be objected to this, that in the complementary colours exhibited by doubly refracted pencils in polarized light, yellow is often found to be complementary to purple, and blue to orange But in neither of these pairs of colours is the spectrum divided in the manner above indicated, and, moreover, in many instances yellow and blue *are* found as complementary colours in the oppositely polarized pencils, of which examples will be found in the scale of tints produced by sulphate of barytes in my paper "On the Action of Crystallized Bodies in Homogeneous Light" (Phil Trans 1820, Table I) "Rich yellow" appears also as opposed to "full blue" in the scale of complementary tints exhibited by mica in my "Treatise on Light" (Encyc Metrop., art 507). It is not asserted that either a good yellow or a good blue cannot be produced otherwise than in a particular manner, but that they *can* be produced *in* that particular manner, and that *when* so produced, their union affects the eye with no sensation of greenness

Let two very narrow strips of white paper, A, B, be placed parallel to one another in sunshine, so as to be seen projected on a perfectly black ground (a hollow shadow), and viewed through a prism having the refracting edge parallel to them, the refraction being towards the eye, and let the nearer B be gradually removed towards A, so that the red portions of B's spectrum shall fall upon the green portion of A's Their union will produce *yellow*, or, if too far advanced, *orange* On the other hand, it will be seen that the yellow space in B's spectrum on which the blue of A's falls is replaced by a streak of white,—whiteness, and not greenness, being the resultant of the joint action of these rays on the retina If the strips be made wedge-shaped, tapering to fine points, and A being still white, B be made of paper coloured with the yellow chromate of mercury before mentioned, the whiteness of the streak where the blue of A mixes with the yellow of B near the pointed extremities will be very striking

There is a certain shade of cobalt-blue glass which insulates, or very nearly so, a definite yellow ray from the rest of the spectrum, suppressing the orange and a great deal of the green If the spectrum of B, formed and coloured as last described, be viewed through this glass, a very well-defined image of it, clearly separated from its strong red and very faint blue images, will be seen As the glass in question allows blue rays to pass, the white object, A, besides its definite yellow image, will form a broad blue, indigo, and violet train nearer to the eye. Now let B be gradually brought up towards A, so that the violet, indigo, and blue rays of this train shall coincide in succession with the yellow image of B,—*no sensation of greenness will arise at any part of its movement* Again, if a white card be laid down on a black surface, the edge nearest the eye, when refracted towards the spectator by a prism, will of course be fringed with the more refrangible half of the spectrum Let this be viewed through such a glass, and in the blue space so seen introduce one half of a narrow rectangular slip of paper thus coloured, having its upper edge in contact with the lower edge of the white card, the other half projecting laterally beyond the card. In this arrangement the definite image of the yellow paper insulated by the glass will be seen divided into a *yellow* half, projecting beyond the blue fringe, and a purplish- or bluish-white one within it, hardly to be distinguished from the image of the white paper, of which it seems a continuation, and which through the glass in question appears a pale blue This same purplish tint was observed to arise also under the following circumstances ·—Laying down in a good diffused light a paper of an exceedingly beautiful ultramarine blue, and beside it, and somewhat overlapping it, another coloured with the same yellow chromate, I set upon the line of junction a sheet of glass inclined to the plane of the papers upwards towards the eye, so as to allow the blue to be seen by transmitted light, while the yellow reflected from the glass was at the same time received into the eye By varying the inclination of the glass, the yellow reflexion could be made more or less vivid, so as either to be nearly imperceptible or quite to *kill* the blue of the paper But at no stage of its intensity, gradually increased from one to the other extreme, was the slightest tendency to greenness produced The colour passed from blue to yellow, not through green, but through a pale uncertain purplish tint, not easy to describe, but as remote from green as could be well imagined

Of course in all such experiments one eye only must be used Stereoscopic superposition of colour, which at first sight would appear readily available, does not satisfy the requisite conditions, and yields no definite results

The conclusions from these facts may be summed up as follows —1st. That in no case can the sensation of green be produced by the joint action on the eye of two lights, in neither of which, separately, prismatic green exists, 2ndly That the joint action of two lights, separately producing the most lively sensations of blue and yellow, does not give rise to that of green, *even when one of them contains in its composition the totality of green light in the spectrum*, and, 3rdly That all our liveliest sensations of yellow are produced by the joint action of rays, of which those separately exciting the ideas of red and green form a large majority, and that a decided yellow impression is produced by the union of these only

From these premises it would seem the easiest possible step to conclude the non-existence of yellow as a primary colour But this conclusion I am unable to admit in the face of the facts,—1st, that a yellow ray, incapable of prismatic analysis into green and red, may be shown to exist, both in the spectrum and in flames in which soda is present, and 2ndly, that neither red nor green, as sensations, are in the remotest degree suggested by that yellow in its action on the eye Whether under these circumstances the vision of normal-eyed persons should be termed trichromic or tetrachromic, seems an open question

That Mr. Pole's vision is *di*chromic, however, there can be no doubt If I could ever have entertained any as to the correctness of the views I have embodied of the subject in that epithet, after reading all I have been able to meet with respecting it, this paper would have dispelled it That he sees blue as we do, there is no ground for doubting, and I think it extremely likely that his sensation of whiteness is the same as ours Whether his sensation of yellow corresponds to ours of yellow, or of green, it is impossible to decide, though the former seems to me most likely

One of the most remarkable of the features of this case, and indeed of all similar ones, is the feebleness of the efficacy of the red rays of the spectrum in point of illuminating power, which certainly very strongly suggests an explanation drawn from the theory of three primary coloured *species of light*, to one of which the colour-blind may be supposed absolutely insensible. Mr Pole himself evidently leans to this opinion I had satisfied myself, however, in the case of the late Mr Troughton, that the *extreme* red—that pure and definite red which is seen in the solar spectrum only when the more luminous red is suppressed, and in which I cannot persuade myself that any yellow exists, was not invisible to him,—though of course not seen as *red*, and on supplying Mr Pole with a specimen of a glass, so compounded of a cobalt-blue and a red glass as to transmit positively no vestige of any *other* ray, but *that* copiously, so that a candle seen through it appears considerably luminous, and the window-bars against a cloudy sky are well seen if other light be kept from falling on the eye,—I am informed by him that he saw through it "gas, *fire* and other strong lights with perfect distinctness," and that the colour so seen is a "very deep dark yellow" Now it seems to me impossible to attribute this to any minute per-centage of yellow light of the same refrangibility, which this can be supposed to contain. The purity of its tint is extraordinary, and its total intensity so small, that supposing it reduced to one tenth of its illuminating power by the suppression of the whole of its primary red constituent, I cannot imagine that any gas-flame or fire-light would be visible through it, or any other luminous body but the sun

Still it remains a fact, however explained, that the red rays of the spectrum generally are to the colour-blind comparatively but feebly luminous Mr Pole speaks of red in more places than one as "*a darkening power*," and in the letter I have received from him in reply to my query as to the visibility of light through the red glass above mentioned, he insists strongly on its action as *darkness* This, however, can only be understood of the effects of red powders in mixture, and not of red *light*, and as, to our eyes, an intense blue powder, such as prussian blue, has, besides its colorific effect, a violent darkening one (owing to its feeble luminosity), so, to the colour-blind, red powders, when added to others, contribute but little light in proportion to the bulk they occupy in the mixture, and therefore exercise a darkening power by displacing others more luminous than themselves I think it therefore very probable that red appears to the colour-blind as yellow-black does to the normal-eyed, or, in other words, that our higher reds are seen by them as we see that shade of brown which verges to yellow—that of the faded leaf of the tulip-tree for instance Now it is worthy of remark, that it is very difficult for the normal-eyed to become satisfied that the browns are merely *shades* of orange and yellow *Brownness* (such at least has always been my own impression) is almost as much a distinct sensation as greenness, so that I am not at all surprised at the expression in § 22, that the "sensation of red as a *dark yellow* is certainly very distinct from *full yellow*," or that a colour-blind person should, after long and careful investigation, arrive at the conclusion that red is not to him a distinct colour I find all this completely applicable to my own perception of the colour brown

Mr Pole (§ 11) appears to lay great stress on the fact, that in a closed colour circle in which red, yellow, and blue are so arranged that each shall graduate into both the others, there occurs in the space where red and blue graduate into each other, "a hue of red which is to him absolutely insensible," and that this red corresponds *not* to that colour which, under the name of carmine, offers to the normal-eyed the *beau-ideal* of redness, but what they term "crimson" Invisibility, as an element of colour, must not here be confounded with invisibility as light It is certain that he *sees* the crimson It is not to him black, but (just what it ought to be on the supposition that his vision is dichromic, and the union of his colours produces white) a neutral, obscure grey, grey being only an abbreviated expression for feeble illumination by white light. In a circle coloured with three elements graduating into each other, there is no neutral point—none, that is, where whiteness or greyness can exist, but when coloured with only two elements, such as yellow and blue (*positive* yellow and blue, that is, whose union produces white, not green), there are of necessity two neutral points which would be both equally white, *i e* equally luminous, if the two extremities of each of the coloured arcs graduated off by similar degrees But this not being the case with the yellow arc, one of its ends to the colour-blind corresponding to a continuation of the red, and so being deficient in illuminating power, the point of neutrality will be that where a feebler yellow is balanced by a feebler blue, and will therefore be less luminous, *i e* less white or more grey than the other neutral spot It is evident, from the general tenor of Mr Pole's expressions throughout this paper, that his ideas on the subject of colour are gathered mainly from the study of pigments and absorptive (i e *negative*) colours, and not from that of prismatic (or *positive*) ones In other words, his language is that of the painter, as distinguished from the photologist, the distinction consisting in this—that in the former colour is considered in its contrast with whiteness, in the other with blackness, and thus it is that black is considered by many painters as an element of colour, as whiteness necessarily is by photologists

I may perhaps be allowed to add a few words as to the statistics of this subject. Dr. Wilson gives it as the result of his inquiries, that one person in every eighteen is colour-blind in some marked degree, and that one in fifty-five confounds red with green. Were the average anything like this, it seems inconceivable that the existence of the phenomenon of colour-blindness, or dichromy, should not be one of vulgar notoriety, or that it should strike almost all uneducated persons, when told of it, as something approaching to absurdity Nor can I think that in military operations (as, for instance, in the placing of men as sentinels at outposts), the existence, on an average, of one soldier in every fifty-five unable to distinguish a scarlet coat from green grass would not issue in grave inconvenience, and ere this have forced itself into prominence by producing mischief Among the circle of my own personal acquaintance I have only known two (though, of course, I have heard of and been placed in correspondence with several), and a neighbour of mine, who takes great delight in horticulture, and has a superb collection of exotic flowers, informs me that among the multitude of persons who have seen and admired it, he does not recollect having ever met with one who appeared incapable of appretiating the variety and richness of the tints, or insensible to the brilliancy of the numerous shades of red and scarlet It may be, however, that the percentage is on the increase—certainly we *hear* of more cases than formerly, but this probably arises from the fact of this, like many *other subjects, being made more generally matter of conversation.*

In further reference to the question of the superposition of colours in the spectrum, or of the intrinsic compositeness of rays of definite refrangibilities, I may mention a phenomenon which I have been led to notice in the prosecution of some experiments on the photographic impressions of the spectrum on papers variously prepared, which appeared to me, *when first noticed*, quite incompatible with the simplicity of those rays at least which occupy the more luminous portion of the spectrum, extending between the lines marked D and E by Fraunhofer, and clearly to demonstrate the presence of green light over nearly the whole of that interval. In these experiments the spectrum formed by two Fraunhofer flint prisms, arranged so as to increase the dispersion, and adjusted to the position of least deviation for the yellow rays, was concentrated by an achromatic lens, and received on the paper placed in its focus, which could be viewed from behind. A series of white papers impregnated with washes of various colourless or very slightly coloured chemical preparations, and dried, were exposed, and the spectrum being received on them, and the centre of the extreme red image as viewed through a standard glass, adjusted to a fiducial pinhole, a sensitizing wash of nitrate of silver, or any other fitting preparation, was copiously applied to the exposed surface while under the action of the light. Now, under these circumstances, I uniformly found that whereas the spectrum viewed from behind through the paper exhibited all over the space in question a dazzling very pale straw-yellow, hardly distinguishable from white, yet as the photographic action proceeded, and the translucency of the paper began to be somewhat diminished also by incipient drying, very nearly the whole of that space became occupied by a full and undeniable green colour, so as to give the idea of a distinctly four-coloured spectrum—red, green, blue, and violet, the yellow being in some instances almost undiscernible, and in others limited to a mere narrow transitional interval rather orange than yellow It was at the same time evident that a great extinction of light (illumination independent of colour) had also been operated, the vivid glare of the part of the spectrum in question being reduced to a degree of illumination considerably inferior to the red part, or, at all events, not much superior The change of colour was far greater than could be attributed to any effect of contrast, and was proved decisively not to be due to that cause by hiding the adjacent red and blue when the green remained unaffected in apparent tint

When, for the photographic preparations wetted as described, ordinary, dry, coloured papers were substituted, the change of colour in question was always produced whenever the thickness of the paper and its absorptive power were not such as to destroy or very much enfeeble the more refrangible light Taking, as a term of comparison, a purely white, wove, writing-paper, I found that the substitution of writing-paper, tinted with the ordinary cobalt blue commonly met with, sufficed to give a very great extension of the green, almost to the extinction of the yellow, while, when the papers used were pale-yellow or clay-coloured, answering to the tints called "buff" or "maize" (nearly approximating to Chevreul's *orangé* 4 and 3), and which might naturally have been expected to transmit yellow rays more abundantly at all events than the blue, the spectra (viewed at the back of the papers) were particularly full and abundant in green, occupying the whole of the debateable ground In the case of the former, a narrow yellow space was seen, and the blue was very much enfeebled, and separated from the green by a very perceptible suddenness of transition. With the latter the green was finely exhibited, and the yellow confined to a narrow orange-yellow border: the blue and violet much enfeebled.

On further considering these facts, there seemed to be but three ways of accounting for them :—1st, by the effect of contrast. This

I consider to be disposed of by the suppression of the adjacent colours, as recorded above 2ndly, by extinction of a *yellow element* of colour over the space DE, allowing a substratum of green to survive; or, which comes to the same, by the extinction of the red element over the same space, which, by its combination with (an assumed elementary) green, produced the original brilliant straw-yellow And 3rdly, by admitting as a principle, that our judgment of colours absolutely, *in se*, and independent of contrast, is influenced by the *intensity* of the light by which they affect the eye, and that very vivid illumination enfeebles or even destroys the perception of colour. As the apparent change of colour from pale-yellow to green in the cases above related was always accompanied with a great diminution of general intensity, it occurred to me to produce such diminution by optical means, which should operate equally on all the coloured rays, and diminish all their intensities in the same ratio This was accomplished by viewing the spectrum (as projected on purely white paper) by reflexion on black glass, or by two successive reflexions in different planes, and I found the very same effect to take place That portion DE of the spectrum which in the unreflected state appeared dazzlingly bright and nearly colourless, was seen by one such reflexion, and still more so by two, green. The extension of the green region was greater, and the limitation of the yellow portion more complete, according to the amount of illumination destroyed by varying the angles of incidence on the glasses. When much enfeebled by two cross reflexions, the aspect of the spectrum was that represented in Chevreul's coloured picture of it from the line A to H. When enfeebled by other means, as by viewing the spectrum thrown on a blackened surface, the effect was exactly the same.

The last of our three alternatives, then, would appear to be established as the true explanation, and in respect of the second, it is eliminated by the consideration that neither the slight degree of coloration in the bluish papers, or the tint of the pale-yellow ones which effected the change, would give rise to so great a *preferential* extinction of yellow or red rays as an explanation founded on that alternative would require The phenomenon is certainly a very striking one, and has created great surprise in those to whom I have shown it.

XII. "On the Laws of Operation, and the Systematization of Mathematics." By ALEXANDER J. ELLIS, Esq., B.A., F.C.P.S. Communicated by ARCHIBALD SMITH, Esq., M.A. Received May 26, 1859.

(Abstract.)

The object of the following investigation is to give a firmer basis to the calculus of operations, to assign the strict limits and connexion of the mathematical sciences, and to found them upon purely inductive considerations, without any metaphysical or *a priori* reasoning.

Starting with the indemonstrable but verifiable hypothesis, that objects exist external to the subject, we recognize equality as existing between objects with common and peculiar properties, in respect of their common properties. Operations, which, when performed on equal objects, produce equal objects as their result, are recognized as equal, in respect to the common properties considered in the equalities of the objects. When one operation is performed on an object, and another on the resultant object, the single operation by which the first object is transformable into the last is regarded as the *product* of the other two, the order of succession being important. When the resultant object is the same as the original operand, the product of the operations is termed *unity*. When two operations performed on the same object produce different resultant objects, the operation of transforming one of these resultant objects into the other, is regarded as the *quotient* of the two former operations. Two operations are termed *reciprocal* when their product is unity. Hence the quotient of two operations is the product of the one and of the reciprocal of the other. When two objects are combined in any manner so as to produce a third, and the two first are formable from any fourth by two known operations, the single operation by which the third object can be also formed from the fourth, is termed the same combination of the two first operations. From this we gain the conception of *null* or *zero*, as the operation of annihilating any object in respect to any place. The product of a combination of two operations and a third operation, is the same combination of the products

of each of the combined operations severally and the third operation, in the particular order thus specified, provided all the operations and products are performable on the same operand

The above general conceptions and laws of combined operations hold for any operations whatsoever with their appropriate operand objects, but the nature of the operations and operands requires especial study In *mathematics*, objects are only considered with respect to their three most general properties first, as contemplatable in discontinuous succession, whence number and *Arithmetic*, secondly, as contemplatable in continuous succession, whence extension and *Geometry*, and thirdly, as contemplatable in a continuous succession bearing a relation to another continuous succession, whence motion in time and *Mechanics* The problem of mathematics is, first, to discover the laws of these successions as respects results (that is, statically), by means of considerations drawn from contemplating operations (that is, dynamical), secondly, to investigate the relations of these laws, giving rise to statical algebra, thirdly, to reduce all dynamical to statical laws, as in dynamical algebra, and fourthly, to make the expression of all the results dependent on the most simple, viz those of common arithmetic The purpose of the problem is to prepare the mind for the further investigation of nature, and to increase practical power immediately.

In *Arithmetic* we conceive objects spread out in a *scale*, and by aggregating those contained between any one and the beginning of the scale, form statical groups, whose distinctive character is derived from the scale The operation by which any group is formed from the first object is termed an *integer*, the especial laws of which are next investigated All objects being interchangeable in respect to discontinuous succession, an aggregate is not changed by altering the disposition of its parts. This leads to the first two *laws of commutation and association in addition* The possibility of arranging objects at once in two horizontal directions, and a third vertical direction, leads to the *laws of commutation and association in multiplication* Combining these with the two former, we have the *law of commutative distribution* From the laws of association in multiplication is immediately deduced the *law of repetition* or indices

Having obtained these laws, we proceed to study their relations in the *algebra of integers*, first, statically, in order to reduce all results to the form of a numerical integer, secondly, dynamically, considering the effect of a variation in the integer employed This leads to the conception of a *formation* (Lagrange's "analytical function"), as a combination of a fixed and independently variable integer Such a combination is, therefore, also itself dependently variable The *inversion of formations*, whereby the independent variable is expressed as a formation of the dependent variable, immediately engages our attention The inversion of a sum leads to a difference, with the limitation that the minuend should be greater than the subtrahend The inversion of a product leads to a quotient, with the limitation that the dividend should be a multiple of the divisor The inversions of a power lead to the root and logarithm, with increasing limitations The study of discontinuous objects then allows the application of these inversions to the solution of problems in common life.

The operation by which any group in the arithmetical scale already described is formable from any other group in the same scale, leads to the conception of a *fraction*, necessarily expressible, according to the general laws of operation, as the quotient of two integers The operands of such operations must admit of being separated into certain numbers of equal parts, or rather, in order that they may admit of *any* fractional operation, into *any* number of equal parts Thus discontinuous approaches continuous succession. The *laws of fractions* are the same as the laws of integers, provided the indices used are all integers The object of the statical *algebra of fractions* is to reduce all combinations of numerical fractions to numerical fractions The inversion of formations is less limited than before There is the same limitation respecting differences, but none respecting quotients The attempt to convert all fractions into radical fractions (whose denominators are some powers of the radix of the system of numeration), leads to the conception of convergent infinite series, and hence allows an approximation to the inversion of a power with a constant index

In *Geometry*, the notion of continuous succession or extension is derived from the motion of the hand, which recognizes separable but not separated parts. This motion gives the conception of surfaces, which by their intersections two and two, or three and three, give lines and points. Recognizing a line as the simplest form of extension, we distinguish the straight lines, which coincide when rotated about two common points, from the curves, which do not These straight lines are shown to be fit operands for the integer and fraction operations By moving one coinciding line over another so as to continue to coincide (by sliding), or to have one point only in common (by rotating), or no points in common (by translation), we obtain the conceptions of angles and parallels, which suffice to show that the exterior angle of a triangle is equal to the two interior and opposite, and that two straight lines meet or not according as the exterior angle they make with a third is not or is equal to, the interior angle Angles are then considered statically as amounts of rotation not exceeding a semi-revolution Proceeding to examine the relations of triangles and parallelograms, we discover the operation of taking a fraction of a straight line, and therefore of a triangle and of any rectilineal figure We see that this operation is, in fact, the same as that of altering a third line into a fourth, so that the multiples of the third and fourth, when arranged in order of magnitude, should lie in the same order as those of the first and second when similarly arranged The relation of two magnitudes, with respect to this order, we term their *ratio*, and the equality of ratios *proportion* The inversion and alternation of the four terms of a proportion are now investigated The operation of changing any magnitude into one which bears a given ratio to it, is called a *tensor* The *laws of tensors*, being investigated, are shown to be the same as those of fractions. They, however, furnish the complete conception of infinite and infinitesimal tensors, by letting one or other of the magnitudes by which the ratio is given become infinite or infinitesimal. Thence is developed the law, that tensors differing infinitesimally are equal for all assignables Consequently tensors may be represented by convergent series of fractions. The *algebra of tensors* allows of the inversion of a sum with the same limitation as in the case of fractions, the complete inversion of a product of tensors, and the practical inversion of a power with a constant integral index. This algebra applied to geometry allows of the investigation of all statical relations, that is, of all the *geometry of the ancients*, in which magnitudes alone were considered, without direction In respect to areas, the consideration of the parallelogram swept out by one straight line translated so as to keep one point on another straight line, leads to an independent *algebra of areas*, in which the generating lines are considered immediately The laws of the relations of lines thus discovered, are shown to be identical with the laws of the relations of tensors Consequently, with certain limitations, the whole of the algebra of tensors may be interpreted as results in the algebra of areas. This leads to a perfect conception of the principle of *homonomy*, or dissimilar operations having the same laws, and consequently the same algebra

In *dynamical* or modern *geometry*, all lines are considered as in construction, having initial and final points If the initial points of any two straight lines are joined to a third, not on either, and the two parallelograms be completed, the lines drawn from the point parallel to the given lines are dynamically equal to them, if these last lie on each other, the first two lines have the *same direction*, if the last have only one point in common and lie in the same straight line, the first have *opposite directions*, and if the last do not lie in the same straight line, the first have *different directions*, and the angle between the last is the angle between the first lines Similar definitions can be given of direction in the case of angles and circular arcs. If from the final point of any line we draw a line equal to a second, and join the initial point of the first with the final point of the line thus drawn, we are said to *append* the second to the first, and the joining line is called the *appense* of the other two. The *laws of appension* are shown to be the same as those of addition, and are hence expressible by the same signs of combination, the difference in the objects combined preventing any ambiguity. We thus get the conception of a point as an annihilated line

The tensor operation, considered dynamically, leads to the operation of changing a line dynamically so that it should bear the same relation to the result as two given lines bear to each other in magnitude and direction This assumes three principal forms according to the difference of direction. If there is no difference of direction, the operation is purely a tensor If the directions differ by a semi-revolution, the rotation of one line into the position of the other may take place on any plane The operation is then termed a *negative scalar*, the tensor, which includes the operation of turning through any number of revolutions, is distinguished as a *positive scalar* If the rotation be through any angle, but always on the same plane, the operation is here termed a *clinant* If the rotation may take place on any variable plane, the operation is a *quaternion*

The *laws of scalars* are immediately proved to be the same as those of tensors, but in addition they introduce the idea of negativity This enables us in the *algebra of scalars*, to invert a sum generally, and thus allows of a perfect inversion of the first two formations But a power with a fixed integral exponent can only be inverted on certain conditions This partial inversion, however, leads to a solution of quadratic equations, and to a proof that formations consisting of a sum of integral powers, cannot be reduced to null by more scalar values of the variable than are marked by its highest exponent Hence if such a formation is always equal to null, all the coefficients of the variable must be null We thus obtain the method of indeterminate coefficients, by which we are enabled to discover a series which obeys the laws of repetition with respect to its variable, and becomes equal to a power when its variable is an integer This enables us to define a power with any index, as this series, and hence to attempt the inversion of powers with variable indices, which we succeed in accomplishing under certain conditions This investigation introduces the logarithm of a tensor, powers with fractional and negative exponents, and the binomial theorem for these powers. It also induces us to consider the *laws of formators*, or the operations by which a formation of any variable is constructed They are shown to be commutative and associative in addition, associative in multiplication, directly distributive and repetitive, but not generally commutative in multiplication, nor even inversely distributive. When formators are commutative in multiplication and distribution, they are entirely homonomous with scalars, which may even be considered as a species of formators. The results of the former investigation, therefore, show that logarithms, fractional and negative powers, and the binomial theorem hold for these commutative formators

The necessity of tabulating logarithms and of approximating to the solutions of equations, leads to the consideration of a method of deriving consecutive values of formations for known differences of the variable, and of interpolating values of the same formation for intermediate values of the variable, that is, the *algebra of differences* Considering the two operations of altering a formation by increasing the variable, and taking the difference between two different values of the formation (of which operations the first is necessarily unity added to the second), we regard them as formators, and immediately apply the results of that algebra, which furnishes all the necessary formulæ For approximating to the roots of equations, we require to consider the case where the variable changes infinitesimally, thus founding the *algebra of differentials*, which is, in fact, a mere simplification of that of differences, owing to all the results being ultimately calculated for assignables only Finally, to find the alteration in a formation of commutative formators, when the variable formator is increased by any other formator, we found the *algebra of derivatives*

In applying the results of *scalar* algebra to *geometry*, we start with the fundamental propositions that the appense of the sides of an enclosed figure taken in order is a point, and that when the magnitude and direction of the diagonal of a parallelogram or parallelopipedon, and lines parallel the sides which have the same initial point as the diagonal, are given, the whole figures are completely determined In order to introduce scalars, a unit-sphere is imagined, with its radii parallel to the lines in any figure, and in known directions Any line can then be represented as the result of performing a scalar operation on the corresponding radius

The first object is to reduce the consideration of angles to that of straight lines, by the introduction of cosines and sines, which are strictly defined as the scalars represented by the relation of the abscissa to the abscissal radius, and the ordinate to the ordinate radius respectively These definitions immediately lead to the relations between the cosines and sines of the sums of two angles, and those of the angles themselves, whatever be their magnitude or direction, and thus found *goniometry*

Defining a *projection* of any figure on any plane to be that formed by joining the points on that plane corresponding according to any law with those of the figure, we have the fundamental relation that, if the first, and therefore the second figure is enclosed, the appense of the sides of the second in the order indicated by the sides of the first, is a point The orthogonal projection of any figure, by means of planes drawn perpendicular to any line, being all in one line, each projection can be represented as the result of a scalar operation performed on the same unit radius, and hence this projection leads to one invariable relation between scalars By choosing three lines at right angles to each other on which to project, we obtain three scalar relations from every solid figure If the figure is plane, then by projecting on a line and on a perpendicular to that line, we get two scalar relations

Applying these results to *transversals*, where a line parallel to one unit radius cuts several other unit radii, produced either way if necessary, we obtain, by considering *two* intersected radii, the results of *trigonometry*, and by considering *three* or *four* intersected radii, those of *anharmonic ratios*

As any line drawn from the centre of the unit-sphere may be considered as the appense of three lines drawn along or parallel to three given unit radii, it may be expressed as the sum of the results of three scalar operations performed on these radii respectively By properly varying these three scalars, the final point of the line may be made to coincide with any point in space But if there be a given relation between the scalars, then the number of points will be limited, and the whole number of the points constitutes the locus of the original concrete equation referred to the accessory abstract equation The consideration of this entirely new view of *coordinate geometry* is reserved for a second memoir

Proceeding next to the *laws of clinants*, we readily demonstrate that they are the same as the laws of scalars, they introduce a new conception, however, that of rotating through an angle not necessarily the same as a semi-revolution, that is, of a plane versor By the concrete equation of coordinate geometry, it is immediately shown that all clinants can be expressed as the sum of a scalar, and of the product of a scalar by a fixed, but arbitrarily chosen versor. The simplest versor to select is the quadrantal versor, which, under the name of quadrantation, is now studied The two addends of a clinant, considered as a sum, are called its scalar and vector, its two factors, considered as a product, are its tensor and versor. The laws of these parts are then studied.

The statical *algebra of clinants* has for its object the reduction of all combinations of clinants given in the standard form of the sum of a scalar and vector, to a clinant of the same form. The application of this to the series obtained for a general scalar power, leads to two series, called cosines and sines of the variables, as distinguished from the goniometrical cosines and sines of an angle, with which they are ultimately shown to have a close connexion, which can be rendered most evident by assuming as the unit-angle that subtended by a circular arc of the length of its radius. Studying these series quite independently of these relations to angles, we discover that they bear to each other the same relations as the goniometrical cosines and sines, and that if the least tensor value of the variable for which the cosine series becomes null, is known, all its other values can be found by multiplying this by four times any scalar integer. This last product must be added to the least tensor value of the variable for which both the cosine or the sine series become equal to given scalars, in order to find all the solutions of such equations. Supposing the values of such series tabulated by the method of differences for all scalar values of the variable, so that such least tensor values can always be found, we are now able to assign the meaning of any power whose base and index are both clinants, and the logarithm of any clinant. This enables us to invert completely all the simple formations, sum, product, power with variable base and constant index, or constant base and variable index, and hence to solve all equations of four dimensions with clinant coefficients, and to show that every formation consisting of a sum of integral powers with clinant coefficients, can be expressed as a product of as many simple formations as is determined by the highest index of the variable. The cosine and sine series can also be generally inverted. The versor of any clinant having a known angle (which is always equal to the cosine of its angle added to the product of the sine of its angle into a quadrantal versor), can now be shown to equal the cosine series added to the sine series multiplied by a quadrantal versor, when the variable of the series is the scalar ratio of the angle of the clinant to the angle subtended by a circular arc equal to its radius. From this the ratio of the circumference to the diameter of a circle is shown to be twice the least tensor value of the variable, for which the cosine series is equal to null, and as that value can be readily assigned in a convergent series, the former ratio is determined. The same investigation shows the relation already mentioned between the goniometrical cosines and sines, and the cosine and sine series.

Clinant algebraical geometry allows us to interpret all results of clinant algebra when referred to lines on one plane. It thus furnishes a complete explanation of the "imaginary" points and lines in the theory of *anharmonic ratios*, when viewed in relation to the unit radii, as already explained In the case of *coordinate geometry* of two, and even three dimensions, the possibility of interpreting the results of a clinant operation performed on a given unit radius in a given plane, allows us to understand the whole theory of "imaginary" intersections. The theory of *scalar and clinant algebraical coordinate geometry* will form the subject of a future memoir

Proceeding to *quaternions*, we find their laws to be the same as those of clinants while the plane remains unaltered, but if the plane is alterable, they cease to be commutative in multiplication, that relation being replaced by one between certain related quaternions called their conjugates. This makes the *algebra of quaternions* (which is not here systematized, as being too recent) entirely different from that of scalars

In *mechanics* the motion of any point is not considered absolutely as in dynamical geometry, but relatively to some external, constant, independent motion, as the apparent motion of the fixed stars, this gives the conception of time But the necessity of considering the motion not merely of a point, but of a body, gives rise to the comparison of the motions of various bodies, and to a conception of their equality, when the products of their velocities, multiplied by a constant which is always the same for the same body, but different for different bodies, are equal This constant is the mass, which in bodies of the same kind varies as the volume

By considering the case of the mutual destruction of motion, we eliminate time and simplify the problem, thus founding *statics*, and by conceiving the' motion of any body to be destroyed by the application of variable motions equal and opposite to those actually existent, we reduce *dynamics* to statics

June 9, 1859

Sir BENJAMIN C BRODIE, Bart , President, in the Chair

The Annual General Meeting for the Election of Fellows was held this day

The Statutes respecting the election of Fellows having been read, John Bishop, Esq and Edward Brayley, Esq were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected —

Samuel Husbands Beckles, Esq
Frederick Crace Calvert, Esq
Henry J Carter, Esq
Douglas Galton, Esq
William Bird Herapath, M D
George Murray Humphry, Esq
Thomas Sterry Hunt, Esq
John Denis Macdonald, Esq
William Odling, Esq
Robert Patterson, Esq
John Penn, Esq
Sir Robert Schomburgk
Thomas Watson, M D
Bennet Woodcroft, Esq
Lieut -Col William Yolland, R E

The Meeting then adjourned

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION

I. "On the frequent occurrence of Vegetable Parasites in the Hard Structures of Animals." By Professor A KOLLIKER, of Wurzburg. Communicated by Dr. SHARPEY, Sec R S. Received May 30, 1859

As far as I am aware, Quekett has been the first to point out that vegetable parasites, viz *Confervæ*, occur frequently in the skeleton of Corals (Lectures on Histology, vol ii p 153 fig 78 and p 276), but although he mentions in the same place that the *tubuli* described by Carpenter in the shells of Bivalves have also a great resemblance with *Confervæ*, he did not venture any further step, and he adheres to the view of Carpenter, who regards them as a typical structure Some years later, Rose ("On Parasitic Borings in Fossil Fish-Scales," Transactions of the Microscopical Society of London, vol x p 7, 1855) discovered a peculiar tubular structure in fossil fish-scales, which he regarded as being occasioned by parasites, and possibly by Infusoria, but he was not able to give any good proof of this hypothesis The same must be said of E Claparède (Müll Archiv, 1857, p 119), who found similar canals in the test of *Neritina fluviatilis*, and showed that they do not really belong to the shell, without being happier in determining the nature of the parasite, only suggesting that it might possibly be a sponge

Such was the state of things, when Prof Wedl of Vienna and I, independently of each other, took up the question The observations of Wedl, which concern only the parasites of the shells of Bivalves and Gasteropods, were communicated to the Vienna Academy on the 14th of October, 1858, and are therefore previous to my own, which were presented to our Wurzburg Society on the 14th of May, 1859, but I received Wedl's memoir only on the 16th of May, and may therefore say that my observations, which are also extended over many more groups of animals, were quite independent of those of the Austrian microscopist This being the case, it may be regarded as a good proof of the correctness of our observations and the truth of our conclusions, that we agree in the principal facts, there being only this discrepancy between us, that Wedl calls the parasites *Confervæ*, whilst I regard them as *Unicellular Fungi* The botanists will decide this question better than we, only I beg leave to say, that all the numerous parasites observed by myself were *unicellular*, and that the *sporangia* were quite of the same kind as those of unicellular fungi I may further add, that the frequent anastomoses of the parasitic tubes remind one of the anastomoses observed in the mycelium of some unicellular fungi, whereas such connexions have not yet, so far as I know, been observed amongst the *Confervæ*

I now give a short enumeration of the animals in whose skeleton I observed these vegetable parasites.

1 *Spongiæ*

Two undetermined species of sponges, which I got through the kindness of Mr. Bowerbank, show a great many parasitical tubes in the horny fibres of their skeleton These are most elegant and numerous in one species from Australia, in which the tubes form a superficial network in the outermost parts of the horny sponge-fibres and more straight canals in their interior, and possess a great many round *sporangia*, which in some cases even showed young outgrowths in form of short ramifying tubes

2 *Foraminifera*

In an extensive collection of sections of Foraminifera which I owe to the kindness of my friend Prof Carpenter, there were many genera which showed numerous filaments of fungi in their test itself, viz *Polystomella*, *Orbitolina*, *Heterostegina*, *Amphistegina*, *Calcarina*, *Alveolina*, and *Operculina* The last genus shows best that these parasitic tubes, which sometimes are very large, are quite different from the two kinds of tubes rightly described by Carpenter as belonging to the test itself They generally run at right angles to the finer tubuli, and are easily distinguished from both kinds of typical tubuli by their irregular course, and by their frequent branching, and even anastomosing They are absent in many specimens of the above-named genera, and could not be found in *Cycloclypeus*, *Nummulina*, and *Nonionina*

3 *Corals*

All the genera of Corals which I investigated contained parasitical fungi, viz. *Astræa diffusa*, *Porites clavaria*, *Tubipora musica*, *Corallium rubrum*, *Oculina diffusa*, *Oculina*, sp, *Alloporina mirabilis*, *Madrepora cornuta*, *Lobalia prolifera*, *Millepora alcicornis*, *Fungia*, sp The fungi were most frequent in the genera *Tubipora*, *Astræa*, *Porites*, and *Oculina*, the last three of which contained also many *sporangia*, which in the red coral were very scarce and often wanting

4 *Bivalves*

I agree with Wedl that the tubuli described by Carpenter in the shells of Bivalves are all parasites Many of them agree in every respect with those found in other hard structures of the Invertebrata, of whose parasitical nature there can be no doubt, and even possess *sporangia*, as those of *Thracia*, *Lima*, *Cleidothærus*, *Anomia*, *Ostrea*, *Meleagrina* With respect to those of the genera *Lithodomus*, *Arca*, *Pectunculus*, *Nucula*, *Cardium*, it is true that their straight course and more regular distribution speak in favour of their typical occurrence; but as in some cases true parasites also are very regularly distributed through the shells, there can be no doubt that even these do not really belong to the structure of the shells

5 *Brachiopods*

The test of some *Terebratulæ* shows, besides the large well-known canals, minute tubuli running straight through the fibres A vertical section of *Terebratula australis*, which I got from Prof Carpenter, showed that the minute canals referred to belong to a vegetable parasite of the same kind as those of the Bivalves

6. *Gasteropods*

Nearly all examined Gasteropods, viz *Cerithium tuberculatum*, *Aporrhais pes-Pelecani*, *Turbo rugosus*, *Murex brandaris*, *Murex trunculus*, *Haliotis*, *Vermetus*, *Trochus*, *Littorina littorea*, *Terebra myurus*, *Tritonium cretaceum*, contained vegetable parasites in their shells, and in some these were as numerous as in the Bivalves, and showed also *sporangia* Besides these fungi, the shell of *Trochus* also contained in its most superficial layers unicellular pyriform algæ with green contents

7 *Annelids*

Even in this group the unicellular parasites were found, viz in the calcareous tubes of two *Serpulæ* from the Scotch coast

8. *Cirrhipeds*

The same parasites also occurred very numerously in the shells of a large *Balanus* On the other hand, the genera *Diadema* and *Lepas* were free from them , and with regard to the straight tubes of *Pollicipes* described by Quekett, which also occur in *Tubicinella*, I am inclined to reckon them amongst the typical structures.

9 *Fishes*

The scales of *Beryx ornatus*, from the clay, contain very numerous and pretty parasitic structures, which almost totally agree with those figured by Rose in his fig 5 They undoubtedly also belong to the simplest form of fungi, but are of greater interest, inasmuch as they are fossil and seem to constitute a new genus I was not able to find parasites in any other fish-scales, notwithstanding that I examined scales of all living and many fossil species of Ganoids and many *Teleostei*

These are the facts which I have been able to gather, up to this time I have no doubt that all will ageee with me in regarding this question as one of great interest for the zoologist as well as for the botanist The former will now be obliged to study these parasitical structures as thoroughly as possible, in order to decide which tubular structures of the hard tissues of animals are typical and which are not, and for the botanist a new field of investigation is opened, which not only draws attention by the somewhat strange forms offered for investigation, but is also of great interest in a physiological point of view. It seems to me probable that the parasites dissolve the carbonate of lime of the hard structures into which they penetrate, by means of exudation of carbonic acid, which secretion would seem to take place only at the growing ends of the fungial tubes, as they never lie in larger cavities, but are always closely surrounded by the calcareous mass In some cases, as in the horny fibres of sponges, it seems probable that the parasites simply bore their canals by mechanical force, as is the case when vegetable parasites make their way through the cell-membranes of *Confervæ* or other plants Besides this, it deserves also to be remembered that nearly all the parasites here spoken of occur in marine animals

In concluding this notice, I may further mention that these parasites afford an excellent means for demonstrating the *double-refracting power* of the shells of the several genera mentioned in this communication I was first struck with this fact in examining a horizontal section of *Lima scabra* obtained from Dr Carpenter, and finding that many tubuli appeared double In following this matter, it was easy to show that all the tubuli running in a certain direction, and in an oblique way through the section, appeared simple at the upper surface of it, and became double in the inferior layers, so that the distance of the two images increased with the shortening of the focus When the preparation was inverted, the reverse was the case. The same phenomena as in *Lima* were also observed in *Anomia, Ostrea, Murex truncatus, Turbo rugosus, Tritonium cretaceum*, and *Balanus*, the shells of which animals have therefore all such a structure, that they refract the light in the same way as the well-known double-refracting crystals*

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* According to Brewster (Bibl. Univ de Genève, 1836 ii 182), who seems the only person who has hitherto observed the double-refracting power of a shell, viz of the mother-of-pearl, that shell (*Meleagrina*) shows the same phenomena as the double-axed double-refracting Arragonite, on which question I am not as yet able to give an opinion

II "Researches on the Phosphorus-Bases"—No VI Phosphammonium-Compounds By A W Hofmann, LL D, F R S &c Received June 1, 1859

In several previous communications * I have shown that dibromide of ethylene is capable of fixing either one or two molecules of triethylphosphine, a monatomic and a diatomic bromide being formed, which I have respectively represented by the formulæ—

Monatomic bromide

$$C_{16}H_{19}PBr_2=(C_4H_4)''Br_2+(C_4H_5)_3P=\left[\left.\begin{array}{l}C_4H_5\\C_4H_5\\C_4H_5\\(C_4H_4Br)\end{array}\right\}P\right]Br$$

and

Diatomic bromide

$$C_{28}H_{34}P_2Br_2=(C_4H_4)''Br_2+2\,((C_4H_5)_3P)=\left[\left.\begin{array}{l}(C_4H_5)_2\\(C_4H_5)_2\\(C_4H_5)_2\\(C_4H_4)''\end{array}\right\}P_2\right]''Br_2$$

There are other products formed, resulting from secondary reactions.

It was not quite easy to obtain a sufficiently satisfactory experimental foundation for the diatomic nature of the second compound This substance presents an extraordinary degree of stability, in its general characters it is closely allied to the numerous monatomic bromides, both of the nitrogen- and of the phosphorus-series, which in the course of these researches have come under my consideration Lastly, the oxygenated derivative of the bromide resembles so perfectly the monammonium- and the monophosphonium-bases, that more than once during my experiments I was inclined to doubt the correctness of my interpretation.

There is no direct proof of the diatomic character of the compound Why should we reject the simple formula deducible from experiment? The hydrocarbons $C_n H_n$ are very prone to molecular transformations without change of composition The idea suggested itself, that the diatomic saline molecule might be split into two monatomic saline molecules,

$$[(C_4H_5)_6(C_4H_4)''P_2]''Br_2=2\,([(C_4H_5)_3(C_2H_2)'P]\,Br).$$

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* Proceedings, vol ix pp 285 and 631

It is true C_2H_2 figures in this formula as monatomic, whilst we should expect it endowed with diatomic substitution-power But the connexion between composition and substitution-power is by no means finally settled, in fact, we know of many cases in which, under conditions not sufficiently established, the atomicity of a molecule changes witness the radical "allyl," which is capable of replacing one or three equivalents of hydrogen

But without going this length, the scission of diatomic ethylene into two monatomic molecules may take place in many other ways The transformation of dibromide of ethylene into hydrobromic acid and bromide of vinyl,

$$(C_4H_4)''Br_2 = HBr + (C_4H_3)Br,$$

is a familiar example The splitting of the ethylene-compound into bromide of formyl and bromide of methyl,

$$(C_4H_4)''Br_2 = (C_2H)Br + (C_2H_3)Br,$$

has never been observed, but did not appear altogether unlikely

Our analytical methods are insufficient to distinguish between

$$[(C_4H_5)_3(C_2H_3)P]Br \text{ and } [(C_4H_5)_3(C_2H)P]Br,$$

and what I have represented as a diatomic ethylene-compound might have been, after all, a monatomic bromide—the bromide of formyl-triethylphosphonium, the complementary methyl-compound

$$[(C_4H_5)_3(C_2H_3)P]Br$$

existing possibly among the secondary products of decomposition.

In the presence of these and several similar self-raised objections, by which every observer endeavours to test the truth of his conclusions, I was induced again to appeal to experiment

The prosecution of this line of the inquiry has led me to the discovery of a new class of diatomic bodies, which, while it confirms incontestably the correctness of my interpretation, appears to claim the attention of chemists for several other reasons

I have established, in the first place, that the monatomic bromide

$$[(C_4H_5)_3(C_4H_4Br)P]Br$$

may be readily converted into the diatomic bromide

$$[(C_4H_5)_6(C_4H_4)''P_2]''Br_2$$

by the simple addition of triethylphosphine Nothing is easier than to prove the transformation, the platinum-salt of the two bases presenting a remarkable difference of solubility, and other differences not less striking

To remove every doubt, the bromide, obtained by treatment of the brominetted bromide with triethylphosphine, was converted into the corresponding iodide, which in its properties and composition was found to be identical in every respect with the characteristic iodide, which I have fully described in my last note upon this subject

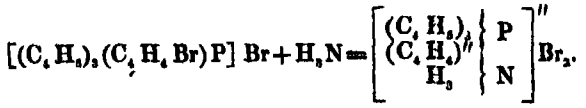
The transformation of the monatomic into what I have represented as the diatomic compound being satisfactorily established, the conclusive experimental demonstration of the diatomic nature of the latter presented itself without difficulty in the conception of bromides containing at once phosphorus and nitrogen, the molecular expression of which would no longer admit of division

This class of dibromides actually exists, they are readily produced by submitting the bromide of the brominetted body to the action of ammonia or monamines instead of triethylphosphine

I have formed as yet only three representatives of this new class of bodies, which I propose to designate as phosphammonium-compounds, their examination is sufficient to fix the character of the class, it would have been easy to construct scores of similar bodies.

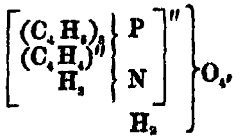
Action of Ammonia upon the bromide of the brominetted body

The two substances, especially when in alcoholic solution, unite with evolution of heat—



$$[(C_4H_5)_3(C_4H_4Br)P]Br + H_3N = \left[\begin{matrix}(C_4H_5)_3\\(C_4H_4)''\\H_3\end{matrix}\left\{\begin{matrix}P\\ \\N\end{matrix}\right.\right]''Br_2.$$

Both the bromide and the corresponding chloride are very soluble, and little adapted for analysis, I have therefore fixed the nature of this body by the preparation and analysis of the platinum-compound. For this purpose the bromide generated in the above reaction was treated with oxide of silver, it is thus converted into a powerfully alkaline solution obviously of the dioxide,



$$\left.\begin{matrix}\left[\begin{matrix}(C_4H_5)_3\\(C_4H_4)''\\H_3\end{matrix}\left\{\begin{matrix}P\\ \\N\end{matrix}\right.\right]''\\H_2\end{matrix}\right\}O_4,$$

which, saturated with hydrochloric acid and mixed with dichloride of platinum, furnished a light-yellow crystalline platinum-salt, recrystallizable from boiling-water, and containing

$$[(C_4H_5)_3 H_3 (C_4H_4)'' PN]'' Cl_2, 2PtCl_2$$

Action of Ethylamine and Trimethylamine upon the bromide of the brominetted body

The phenomena observed with ethylamine and trimethylamine are perfectly analogous These substances furnish, with the brominetted bromide, new and very soluble dibromides, containing respectively

$$\left[\begin{array}{l}(C_4H_5)_3\\(C_4H_4)''\\(C_4H_5)H_2\end{array}\left\{\begin{array}{c}P\\ \\N\end{array}\right.\right]''Br_2 \quad\text{and}\quad \left[\begin{array}{l}(C_4H_5)_3\\(C_4H_4)''\\(C_2H_3)_3\end{array}\left\{\begin{array}{c}P\\ \\N\end{array}\right.\right]''Br_2,$$

which, by treatment with oxide of silver, are converted into the corresponding powerfully alkaline oxides

$$\left.\begin{array}{r}\left[\begin{array}{l}(C_4H_5)_3\\(C_4H_4)''\\(C_4H_5)H_2\end{array}\left\{\begin{array}{c}P\\ \\N\end{array}\right.\right]''\\H_2\end{array}\right\}O_4 \quad\text{and}\quad \left.\begin{array}{r}\left[\begin{array}{l}(C_4H_5)_3\\(C_4H_4)''\\(C_2H_3)_3\end{array}\left\{\begin{array}{c}P\\ \\N\end{array}\right.\right]''\\H_2\end{array}\right\}O_4,$$

and yield, by saturation with hydrochloric acid and precipitation with dichloride of platinum, two splendid platinum-salts crystallizing in long golden-yellow needles, and containing respectively

$$[(C_4H_5)_4 (C_4H_4)'' H_2 PN]'' Cl_2, 2Pt Cl_2, \text{ and}$$
$$[(C_4H_5)_3 (C_2H_3)_3 (C_4H_4)'' PN]'' Cl_2, 2Pt Cl_2$$

By the formation of the phosphammonium-compounds, the nature both of the diammonium- and of the diphosphonium-series appears to me finally established

It will be interesting to ascertain whether the brominetted bromide, when submitted to the action of monarsines and monostibines, will give rise to the formation of phospharsonium- and phospho-stibonium-bases. The solution of this question will not be difficult

III “Notes of Researches on the Poly-Ammonias”—No VI New Derivatives of Phenylamine and Ethylamine By A. W Hofmann Received June 9th, 1859

Some time ago* I communicated to the Royal Society some results obtained in studying the action of dibromide of ethylene upon phenylamine The principal product of this reaction was found to be a well-defined crystalline compound with basic characters By the analysis of the base itself, and of several of its combinations, it had been proved that the formula

$$C_{16}H_9N=\left.\begin{matrix}(C_4H_4)''\\C_{12}H_5\end{matrix}\right\}N$$

is the simplest atomic expression for the new substance, but the action of iodide of methyl and of ethyl upon this body having given rise to compounds

$$C_{34}H_{21}N_2I=\left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\}C_2H_3I$$

and

$$C_{36}H_{23}N_2I=\left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\}C_4H_5I,$$

I was induced to assume the formula

$$C_{32}H_{18}N_2=\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\end{matrix}\right\}N_2$$

as representing the true constitution of the basic body, which thus appears as a diammonia, in which 2 equivs of hydrogen are replaced by 2 equivs. of phenyl, and 4 equivs. of hydrogen by 2 molecules of diatomic ethylene

This view involves the existence of a basic compound,

$$C_{28}H_{16}N_2=\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2,$$

i.e. of a diphenyl-diamine in which only one molecule of diatomic ethylene has been substituted for hydrogen

Experiment has not failed to realize the body pointed out by theory. A mixture of dibromide of ethylene with a large excess of phenylamine (1 vol. of dibromide of ethylene and 4 vols of phenylamine) rapidly solidifies to a crystalline mass Treatment with water removes from this mixture a very considerable proportion of hydrochlorate of phenylamine, leaving a brown resinous substance, which gradually but imperfectly solidifies This substance forms a hydrochlorate which is difficultly soluble in concentrated hydrochloric acid, and which may be readily purified by repeated crystallizations from boiling alcohol The pure hydrochlorate dissolved in water, and mixed with potassa or ammonia, furnishes the free base, which generally separates as an oil, rapidly solidifying into a crystalline substance This may be further purified by repeated crystallizations from diluted alcohol

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* Proceedings of the Royal Society, vol ix p 277.

Analysis, in fact, assigns to this body the formula

$$C_{28}H_{16}N_2 = \left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2,$$

which was confirmed by the analysis of the dichloride—

$$\left[\left.\begin{matrix}(C_4H_4)'\\(C_{12}H_5)_2\\H_4\end{matrix}\right\}N_2\right]''Cl_2,$$

and of the platinum-salt—

$$\left[\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_4\end{matrix}\right\}N_2\right]''Cl_2,\ 2PtCl_2$$

The formation of the new body is obvious

$$4\left[\left.\begin{matrix}C_{12}H_5\\H_2\end{matrix}\right\}N\right] + (C_4H_4)''Br_2 = \left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2 + 2\left(\left[\left.\begin{matrix}C_{12}H_5\\H_3\end{matrix}\right\}N\right]Br\right).$$

Phenylamine	Dibromide of Ethylene	Ethylene-diphenyl-diamine	Bromide of Phenyl ammonium

This substance differs in its physical characters essentially from the base containing 2 molecules of ethylene The former is very soluble in alcohol and ether, the latter being very difficultly soluble, its fusing-point is 59°, the fusing-point of the latter being 157°

In order finally to establish the relation between the body which forms the subject of this note and the base previously described, it remained to prove experimentally that the former, when submitted to the action of dibromide of ethylene, may be readily converted into the latter. Nothing is easier than to accomplish this transformation, which, in the presence of alcohol, is rapidly effected at the temperature of boiling water

Treatment of the product of digestion with water removes the dichloride of ethylene-diphenyl-diammonium, the diethylene-diphenylamine remaining dissolved in the excess of dibromide of ethylene, from which it may be readily extracted by hydrochloric acid

Preparation of the substance in a state of purity, and comparison of its properties with those of the body previously obtained, established beyond a doubt the transformation, which resolves itself into a simple process of substitution --

$$2\left[\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2\right]+(C_4H_4)''Br_2=\left.\begin{matrix}(C_4H_4)_2''\\(C_{12}H_5)_2\end{matrix}\right\}N_2+\left[\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_4\end{matrix}\right\}N_2\right]''Br_2$$

Ethylene-diphenyl-diamine being a secondary diamine, it was not without interest to replace the two remaining hydrogen-equivalents by two monatomic molecules On digesting the base with iodide of ethyl some hours at a temperature of 100°, a beautiful iodide was obtained, crystallizing in well-defined prisms, difficultly soluble in water, but more soluble in alcohol

This substance contains

$$C_{36}H_{26}N_2I_2=\left[\left.\begin{matrix}(C_4H_4)''\\(C_4H_5)_2\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2\right]''I_2$$

Treatment with potassa separates from this iodide the base as a crystalline body fusing at 70°, and resembling in many respects the previous base It contains

$$C_{36}H_{24}N_2=\left.\begin{matrix}(C_4H_4)''\\(C_4H_5)_2\\(C_{12}H_5)_2\end{matrix}\right\}N_2,$$

and forms a beautiful platinum-salt crystallizing in needles of the formula

$$[C_{36}H_{26}N_2]''Cl_2, 2PtCl_2$$

The deportment of phenylamine under the influence of dibromide of ethylene gives a fair illustration of the nature of the substances which are generated, under the influence of diatomic molecules, from primary aromatic monamines

To complete the study of this subject, I have examined, moreover, the action of dibromide of ethylene upon ethylamine, as a representative of the monamines containing an ordinary alcohol-radical

Dibromide of ethylene acts upon ethylamine even in the cold, the products of the reaction varying according to the relative proportions of the two bodies, and according to the temperature Among other products invariably occur the two bromides corresponding to the two salts of the phenyl-compounds mentioned in the previous paragraphs

These substances are the

$$\left.\begin{array}{l}\text{Dibromide of}\\ \text{ethylene-diethyl}\\ \text{diammonium,}\end{array}\right\} C_{12} H_{18} N_2 Br_2 = \left[\left.\begin{array}{c}(C_4 H_4)''\\ (C_4 H_5)_2\\ H_4\end{array}\right\} N_2\right]'' Br_2, \text{ and}$$

$$\left.\begin{array}{l}\text{Dibromide of}\\ \text{diethylene-diethyl-}\\ \text{diammonium,}\end{array}\right\} C_{16} H_{22} N_2 Br_2 = \left[\left.\begin{array}{c}(C_4 H_4)_2''\\ (C_4 H_5)_2\\ H_2\end{array}\right\} N_2\right]'' Br_2$$

I have fixed the composition of the former compound by the analysis of the dibromide of the dichloride and of the base itself, all of which are remarkably well-defined crystalline bodies, and that of the latter by the examination of a well-defined platinum-salt

The first base, separated by the action of anhydrous baryta from the dry bromide, distils as an oily liquid of a powerfully ammoniacal odour, which solidifies into a brittle crystalline mass not unlike fused stearic acid The composition of the body is remarkable It contains

$$C_{12} H_{18} N_2 O_2 = \left[\left.\begin{array}{c}(C_4 H_4)''\\ (C_4 H_5)_2\\ H_4\end{array}\right\} N_2\right]' O_2,$$

and thus constitutes the dioxide of the diatomic metal, ethylene-diethyl-diammonium

The second base is liquid, and boils at 185° It is easily obtained from the dibromide, which, being extremely soluble, may be readily separated from the bromide of the first body I have experimentally established that this body may be readily procured by the action of dibromide of ethylene upon the dioxide previously mentioned

The dioxide,

$$C_{12} H_{18} N_2 O_2,$$

presents considerable interest in a theoretical point of view I have determined the vapour-density of this compound by Gay-Lussac's process Experiment gave the number 2 26 Assuming that the molecule of the body under examination corresponds to 4 volumes of vapour, the theoretical density is 4 62

The extraordinary discrepancy between theory and experiment may be removed in two ways. viz either by halving the formula, or by assuming that the molecule of the dioxide of ethylene-diethyl-diammonium corresponds to 8 volumes of vapour, in either of which cases the theoretical density becomes 2·31, closely agreeing with the experimental number 2·26.

I shall discuss the vapour-densities of the diammonias somewhat more fully in a future communication, but I cannot refrain from pointing out even now, that, by dividing the formula by 2, we arrive at an expression containing 1 equiv of oxygen ($O=8$), which, in the eyes of those who consider the number 16 as the true molecular value of oxygen, must appear perfectly inadmissible

IV "On the Behaviour of the Aldehydes with Acids" By A Geuther, Esq, and R Cartmell, Esq. Communicated by Dr. Frankland. Received June 8th, 1859

[Abstract]

The authors of this paper, with a view of obtaining a series of combinations homologous with those already obtained from glycol by Wurz—viz diacetate of glycol, $\left.\begin{matrix} C_4H_4 \\ C_4H_3O_2 \\ C_4H_3O_2 \end{matrix}\right\} O_4$, and the isomeric body of Geuther from common aldehyde, by the action of anhydrous acetic acid,—have subjected common aldehyde, acrolein, and oil of bitter almonds to the action of hydrochloric, hydriodic, and sulphurous acids

I. *Acrolein,—Metacrolein*

1 *Acrolein and Hydrochloric Acid*

By acting on acrolein, $C_6H_4O_2$, with dry hydrochloric acid gas, a body is formed of the composition $C_6H_5O_2Cl$, resulting from a direct combination of one atom of aldehyde with one atom of the acid This substance is insoluble in water, and can be washed with it in order to free it from any excess of acid or acrolein which may be still present. By drying, which can only be done over sulphuric acid at low temperatures, the body, for which the authors propose the name of hydrochlorate of acrolein, is obtained in a mass of white crystals, presenting a texture like that of velvet It melts at 32° C into a thick oil, having a smell of slightly rancid fat. It is

readily soluble in alcohol or ether, on evaporation of which it remains behind as a thick oil When boiled with water, it remains, as far as can be seen, unchanged. Dilute solutions of the alkalies appear not to act on it Heated with solution of ammonia, in a sealed tube, at 100° C, it is decomposed, chloride of amonium and acrolein ammonia being the result. It does not combine with bichloride of platinum when in solution in alcohol, and very slowly reduces boiling ammoniacal solution of nitrate of silver Heated alone, it decomposes into acrolein and hydrochloric acid By the action of concentrated hydrochloric acid acrolein is set free Dilute sulphuric and nitric acids decompose it likewise, setting acrolein free Heated with hydrate of potash it gives off hydrogen, and there distils at the same time an oily body, which solidifies into magnificent colourless crystals, analyses of which prove it to be an isomeric acrolein, for which the authors propose the name *Metacrolein*

Metacrolein as thus obtained is insoluble in water, but is capable of being recrystallized from alcohol or ether The crystals form very long needles, more especially when melted metacrolein before solidifying is allowed to flow about in a glass tube They resemble very much in appearance the crystals of acetamide, possess a peculiar aromatic smell, and have a taste at first producing a cooling and afterwards a burning sensation They are lighter than water They melt at about 50° C, becoming solid at about 45°C Before melting they are somewhat volatilizable, on which account they can be distilled in the vapour of water On being heated, metacrolein is changed into common acrolein Dilute alkalies do not effect any change in this substance By heating with mineral acids, common acrolein is set free On leading dry hydrochloric acid gas over metacrolein in a bulb-tube, the metacrolein melts and combines with the acid, producing the already-named hydrochlorate of acrolein From this behaviour, the authors believe the acrolein contained in the combination of hydrochloric acid to be metacrolein, and not common acrolein. If metacrolein be viewed as $C_{12}H_8O_4$, the formula of the hydrochloric acid compound would then be $C_{12}H_8O_4, 2HCl$, and the formation of metacrolein may be assumed to take place according to the following equation, $C_{12}H_8O_4, 2HCl + 2KOHO = C_{12}H_8O_4 + 2KCl + 4HO$ The evolution of hydrogen has been found to be the result of a secondary action

2. *Acrolein and Hydriodic Acid*

These substances act very violently on each other if the acid in the gaseous form be led into acrolein, producing a hissing noise, as when red-hot iron is plunged into water The resulting substance is insoluble in alcohol, ether, acids, and alkalies Bisulphide of carbon dissolves out a little free iodine Heated alone, iodine is set free

3 *Acrolein and Water*

Acrolein mixed with two or three times its volume of water, and exposed to the temperature of boiling water for eight days, undergoes a gradual change Acrylic acid is produced, and a resinous substance, soluble in ether, melting at about 60°, and becoming solid at 55°C At common temperatures it is hard and brittle, like resin The per-centage composition of this resin, on analysis, was found to be the same as that obtained by Redtenbacher, and named Desacrylharz*, viz carbon 66 6, hydrogen 7 4

4 *Metacrolein and Hydriodic Acid*

When dry hydriodic acid gas is passed over dry metacrolein, the latter melts, and changes into a heavy yellow solution, resembling in smell and appearance the hydrochlorate of acrolein It can be washed with water, and appears at ordinary temperatures to solidify into crystals, placed over sulphuric acid to dry, it decomposes, becoming brown, and setting iodine free. From the analogy in its formation, this compound can be properly viewed as hydriodate of acrolein

II *Aldehyde*

1 *Aldehyde and Hydrochloric Acid*

Lieben found that by the action of hydrochloric acid on aldehyde, a body of the composition $C_8 H_8 O_2 Cl_2$ was produced, having a constant boiling-point of from 116° to 117° C †

The authors confirm Lieben's paper as to the replacement of O_2 by Cl_2 in two atoms of aldehyde, and have further obtained a new combination, analysis of it giving the formula as $C_{12} H_{14} O_4 Cl_2$, in which two equivalents of oxygen are replaced by the same number of equivalents of chlorine in three atoms of aldehyde By the action of water, this compound, like that of Lieben, is resolved into hydrochloric acid and aldehyde By heat, it is broken up into aldehyde and the body $C_8 H_8 O_2 Cl_2$ The authors propose for it the name protoxychloride of aldehyde

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* Liebig's Annalen, vol xlvii p 145

† Ibid vol cvi p 336

2 *Aldehyde and Hydriodic Acid*

By the action of hydriodic acid on aldehyde a compound is produced that decomposes with water into the aldehyde and the acid again, on which account it could not be purified On heating, it is suddenly decomposed at 70° C, leaving a black resinous residue, which on distillation gave off vapours of iodine In its mode of formation it is analogous to the bodies produced by the action of hydrochloric acid on aldehyde

3 *Aldehyde and Sulphurous Acid—Elaldehyde*

Dry sulphurous acid gas led into anhydrous aldehyde in cold water is absorbed with great avidity, 11 grammes of aldehyde absorbing 19 grammes of the acid, whilst an increase of volume takes place The absorption-coefficient of aldehyde for this acid was found to be 1 4 times greater than that of alcohol for the same, and seven times greater than that of water for it No chemical combination appears to take place, as, on passing a stream of carbonic acid through the fluid at a slightly elevated temperature, almost all the sulphurous acid can be driven out again If aldehyde, saturated with sulphurous acid, be left for about a week at ordinary temperatures in a well-stoppered bottle, it suffers in this time almost a complete change into a body for which the authors propose at present the name Elaldehyde. To obtain it pure, the fluid is mixed with as much water as is necessary to dissolve it up, the acid is saturated by degrees with chalk, and the fluid obtained is distilled so long as oily drops pass into the receiver. The common aldehyde is separated in a resinous form by digesting for some time with solution of caustic soda or potash. By repeated distillation, the elaldehyde can be obtained free from everything but a little water. Analysis gives the formula of this aldehyde as $C_4 H_4 O_2$ It is therefore isomeric with common aldehyde. As it was obtained in quantity by the foregoing method, its properties were further examined. Its boiling point was found to be 124° C, and solidifying-point 10° C Whilst solidifying it likewise starts into crystals, the melting-point of which is also 10° C. The aldehyde here described under the name Elaldehyde is identical with that of Weidenbush* Its mode of production from common aldehyde is the same, its boiling-point likewise agrees with that of the aldehyde of Weidenbush

The elaldehyde of Fehling† the authors believe to be identical with that they have obtained, and also that obtained by Weidenbush That which goes far to prove the identity of the two latter is their vapour-densities That of Weidenbush's is given as 4 58, whilst that of Fehling's is 4 52, both are converted into common aldehyde by heating gently with dilute sulphuric acid, and both crystallize at low temperatures The only material discrepancy between them is the boiling-point of 94° C given by Fehling for elaldehyde, whilst Weidenbush gives the boiling-point of his aldehyde as 125° C

III *Oil of Bitter Almonds*

1 *Oil of Bitter Almonds and Hydrochloric Acid*

This acid does not combine with oil of bitter almonds Experiments made in sealed tubes, heated first to 100° C, and afterwards to 200°, gave no signs of a combination having been effected.

2 *Oil of Bitter Almonds and Hydriodic Acid*

Much better results can be obtained when hydriodic acid is allowed to act on oil of bitter almonds The gas is absorbed, producing an increase of volume and of temperature, and at the same time a little water At the end of the operation two layers appear, of a dark-brown colour The upper one, which is about a sixth part of the quantity of the under one, consists of concentrated hydriodic acid, whilst the under one, a heavy oil, is a compound of iodine and oil of bitter almonds To obtain the substance in a pure state, it was first washed well with water, to remove excess of the acid, next treated with moderately strong solution of sulphite of soda, to remove any excess of oil, lastly, on washing with water, the salt was removed from it It can be dried rapidly over sulphuric acid at a temperature not higher than 20° C. A higher temperature produces gradual decomposition In the preparation of this substance, 6 grammes of oil of bitter almonds absorbed 11 grammes of hydriodic acid gas Analyses of the substance lead to the formula $C_{42}H_{18}O_2I_4$, which will be observed to be 3 atoms of oil of bitter almonds, in which $2(O_2)$ is replaced by $2(I_2)$ The authors propose for it the name Oxyiodide of Benzaldehyde. The substance thus obtained melts at 28° C , and solidifies at about 25° C into almost colourless rhombic plates if rapidly cooled down When in a liquid state, the crystals mostly occur in groups of long needles The colour of the substance in a melted state is brownish yellow, at moderate temperatures, and on standing in the air, it becomes still darker in colour It possesses a smell very much resembling cress It volatilizes at common temperatures, its vapour attacking the eyes powerfully Its vapour at higher temperatures, when carried away by that of water, becomes more and more intolerable, producing a very inflammatory effect on the eyes and nose, which is more painful and permanent than that from acrolein It is insoluble and sinks in water, but can be distilled in the vapour of it Watery solutions of carbonates and sulphites of the alkalies do not act on it Alcoholic solution of potash decomposes it by degrees on heating a little, producing much iodide of potassium, some benzoic acid, and an oily body that remains dissolved in the alcohol, which is not oil of bitter almonds Alcoholic and watery solutions of ammonia change it slowly into iodide of ammonium and oil of bitter almonds. Boiled with solution of nitrate of silver, it yields iodide of silver, and a smell of oil of bitter almonds Concentrated hydrochloric acid changes it by degrees, becoming brown , concentrated sulphuric acid dissolves it on heating, with the separation of iodine

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* Liebig's Annalen, vol. lxvi p 155

† Liebig's Annalen, vol xxvii. p 320

In conclusion, the authors remark that the action of hydrochloric acid on aldehyde may be regarded as consisting in the replacement of two equivalents of oxygen by two of chlorine in one, two, or three atoms of this body · thus,

		Aldehyde containing chlorine	
1 atom of aldehyde	$C_4H_4O_2$	$C_4H_4Cl_2$	
2 ,, ,,	$C_8H_8O_4$	$C_8H_8O_2Cl_2$	Lieben's body
3 ,, ,,	$C_{12}H_{12}O_6$	$C_{12}H_{12}O_4Cl_2$	Protoxychloride of aldehyde

The action of hydriodic acid on oil of bitter almonds gives rise also to a body derived from 3 atoms of this aldehyde, in which 2 (O_2) is replaced by 2 (I_2)

3 atoms of oil of bitter almonds, $C_{42}H_{18}O_6$ — Oxyiodide of Benzaldehyde, $C_{42}H_{18}O_2I_4$.

In the case of acrolein, the action of hydrochloric acid is different, it combines directly with it, no elimination of water taking place If we conceive, however, that, in the action of this acid on common aldehyde, the water which is there produced is the effect of a further decomposition, then we may readily suppose that, if this further decomposition had taken place in the case of hydrochloric acid and acrolein, a body derived from two atoms of acrolein, and having O_2 replaced by Cl_2, corresponding to the second term in the combination of aldehyde and chlorine, would have been the result, thus—

2 atoms of hydrochlorate of acrolein—

$C_{12}H_{10}O_4Cl_2-2(HO)=C_{12}H_8O_2Cl_2$, corresponding to the term $C_4H_4O_2Cl_2$ in common aldehyde

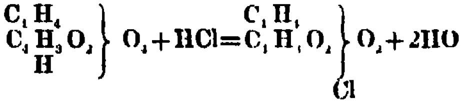
There is a curious connexion which may be mentioned, in this substitution of chlorine for oxygen in aldehyde, between the formula of these bodies containing chlorine, and those of the isomeric modifications of aldehyde.

V. "On the Action of Acids on Glycol" (Second Notice) By Dr. MAXWELL SIMPSON Communicated by Dr FRANKLAND. Received June 29, 1859

Since my last communication to the Society, I have discovered a more convenient process for the preparation of chloracetine of glycol I have ascertained that the monoacetate of glycol is as readily converted into this substance by the action of hydrochloric acid, as a mixture of acetic acid and glycol As the monoacetate is easily obtained, and for this purpose need not be quite pure, it is possible by this method to prepare the body in question on a large scale and with great facility. It is simply necessary to conduct a stream of dry hydrochloric acid gas into the monoacetate, maintained at the temperature of 100° C., till the quantity of oil precipitated on the addition of water ceases to increase The whole is then well washed with water, dried by means of chloride of calcium, and distilled Almost the entire quantity passes over between 144° and 146° C A portion of liquid prepared in this manner gave the following numbers on analysis, which leave no doubt as to its identity —

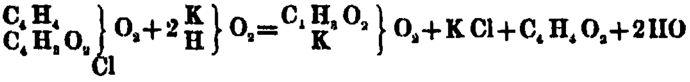
	Theory	Experiment.
C_8	39 18	39 01
H_7	5 71	5 83
O_4	26 14	
Cl	28 97	
	100 00	

The reaction which gives birth to this body may be thus explained —



$$\left.\begin{matrix}C_4H_4\\ C_4H_3O_2\\ H\end{matrix}\right\}O_4+HCl=\left.\begin{matrix}C_4H_4\\ C_4H_3O_2\\ Cl\end{matrix}\right\}O_2+2HO$$

I have made a determination of the vapour-density of chloracetine, and obtained results confirmatory of the formula I have given for this body · experimental vapour-density 4 369, calculated 4 231 for 4 volumes I have also ascertained that oxide of ethylene is formed, and not glycol, when this substance is acted upon by a solution of potash The following equation will explain the reaction —



$$\left.\begin{matrix}C_4H_4\\ C_4H_3O_2\\ Cl\end{matrix}\right\}O_2+2\left.\begin{matrix}K\\ H\end{matrix}\right\}O_2=\left.\begin{matrix}C_4H_3O_2\\ K\end{matrix}\right\}O_2+KCl+C_4H_4O_2+2HO$$

Action of Chloracetine of Glycol on Butyrate of Silver.—Formation of Butyroacetate of Glycol

Equivalent quantities of chloracetine and butyrate of silver were exposed in a balloon with a long neck to a temperature ranging between 100° and 200° C, till all the silver salt had been converted into chloride. The product was then digested with ether, filtered, and the filtered liquor submitted to distillation. As soon as all the ether had been driven off, the thermometer rose rapidly to 180°, and between that temperature and 215° almost the entire quantity passed over. This was fractioned, and the portion distilling between 208°

and 215° was set apart for analysis The numbers obtained lead to the formula $\left.\begin{array}{l}C_4H_4\\C_4H_3O_2\\C_8H_7O_2\end{array}\right\}O_4$, as will be seen from the following percentage table —

	Theory	Experiment I	Experiment II
C_{16}	55 17	54 31	55 58
H_{14}	8 04	8 20	7 97
O_8	36 79		
	100 00		

I also made a determination of the acids by heating a weighed quantity of the ether with hydrate of baryta in the usual manner The quantity of sulphate of baryta obtained indicated 2 2 equivalents of acid for one equivalent of the substance analysed The excess of acid was probably owing to the presence in the ether of a trace of free butyric acid The following equation will explain the reaction which causes the formation of this compound —

$$\underset{Cl}{\left.\begin{array}{l}C_4H_4\\C_4H_3O_2\end{array}\right\}}O_2+\left.\begin{array}{c}C_8H_7O_2\\Ag\end{array}\right\}O_2=\left.\begin{array}{l}C_4H_4\\C_4H_3O_2\\C_8H_7O_2\end{array}\right\}O_4+AgCl$$

In many reactions chlorine replaces, and is replaced by, $H+O_2$, in this it is replaced by the group $C_8H_7O_2$ (equivalent to one atom of hydrogen) $+O_2$

This ether, which I may call butyroacetate of glycol, has a bitter pungent taste It is insoluble in water, but soluble in alcohol It is specifically heavier than water It is a very stable body,—solution of potash, even when boiling, effecting its decomposition with difficulty

I have no doubt that many analogous compounds may be prepared in the manner I have just described

Action of Chloracetine of Glycol on Ethylate of Soda.

In the hope of forming a compound intermediate between diacetate of glycol and diethylglycol, I resolved to try the action of chloracetine on ethylate of soda, thinking that probably the body in question might be generated by the following reaction —

$$\underset{Cl}{\left.\begin{array}{l}C_4H_4\\C_4H_3O_2\end{array}\right\}}O_2+\left.\begin{array}{c}C_4H_5\\Na\end{array}\right\}O_2=\left.\begin{array}{l}C_4H_4\\C_4H_3O_2\\C_4H_5\end{array}\right\}O_4+NaCl.$$

In order to settle this point, I exposed equivalent quantities of these bodies in a sealed balloon to the temperature of a water-bath for about two hours. My expectations, however, were not realized. On opening the balloon, I found that the reaction had proceeded too far, acetic ether having been formed along with the chloride of sodium.

Action of Hydrochloric and Butyric Acids on Glycol.—Formation of Chlorbutyrine of Glycol.

This compound is prepared in the same manner as its homologue, namely by transmitting a stream of dry hydrochloric acid gas through a mixture of equivalent quantities of butyric acid and glycol, maintained at the temperature of 100° C. As soon as the reaction is finished, the product is well washed with water, dried by means of chloride of calcium, and distilled. The greater part passes over between 160° and 182°. This must be rectified, and the quantity distilling between 175° and 182° collected apart. This gave, on analysis, results agreeing with the formula $\left.\begin{matrix} C_4 H_4 \\ C_8 H_7 O_2 \\ Cl \end{matrix}\right\} O_2$, as will be seen from the following table:—

		I	II
C_{12}	47.84	47.76	..
H_{11}	7.30	7.31	
O_4 . . .	21.28		.
Cl	23.58		23.88

The reaction, to which the formation of this body is due, may be thus explained:—

$$\left.\begin{matrix} C_4 H_4 \\ H_2 \end{matrix}\right\} O_4 + \left.\begin{matrix} C_8 H_7 O_2 \\ H \end{matrix}\right\} O_2 + H\,Cl = \left.\begin{matrix} C_4 H_4 \\ C_8 H_7 O_2 \\ Cl \end{matrix}\right\} O_2 + 4\,HO$$

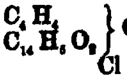
Chlorbutyrine of glycol, as I may call this compound, has a pungent and somewhat bitter taste. It boils at about 190°. Its specific gravity at zero is 1.0854. It is insoluble in water, but freely soluble in alcohol. It is decomposed with difficulty by a boiling solution of potash, but readily by solid potash,—chloride of potassium, butyrate of potash, and oxide of ethylene, being formed.

I have ascertained that acetobutyrate of glycol, the ether I have already described, can be prepared *from* this body as well as *from* chloracetine, by exposing it to the action of acetate of silver The process is the same as that I have already given, with this difference, that the reacting bodies must not be heated above 150° C The ether prepared in this manner gave the following numbers on analysis —

	Theory	Experiment
C_{14}	55 17	56 29
H_{14}	8 04	8 75
O_8	36 79	

The quantity of this substance at my disposal was so small (the greater part of my product having been lost) that I could not purify it completely, hence the experimental numbers do not exactly accord with the theoretical

Action of Hydrochloric and Benzoic Acids on Glycol.—Formation of Chlorbenzoate of Glycol

A mixture of equivalent quantities of glycol and benzoic acid, previously fused and powdered, was exposed to the action of dry hydrochloric acid gas for several hours, the mixture being maintained at the temperature of 100° during the action of the acid, as in the case of the former compounds. The product thus formed presented the appearance of a soft white solid, and contained a considerable quantity of uncombined benzoic acid This was removed by agitating it with hot water, till, on cooling, it no longer became solid, but remained perfectly fluid Finally it was dissolved in alcohol, and precipitated by water. The body thus prepared, and without being distilled, was analysed, having been previously dried in vacuo over sulphuric acid. Another specimen, prepared in the same manner, at a different time, was also analysed, having, however, been previously distilled. During the distillation it was observed that not a drop of fluid passed over till the mercury had risen to 254°, and between that temperature and 270° the entire liquid distilled over. What passed over between 260° and 270° was collected separately, this was the portion analysed The numbers obtained on analysis agree with the formula  $\left.\begin{matrix}C_4H_4\\C_{14}H_5O_2\\Cl\end{matrix}\right\}O_2$, as the following table shows :—

	Theory	Experiment I	Experiment II	Portion distilled
C_{18}	58·54	59·70		58·69
H_8	4·87	5·01		5·31
O_4..	17·35			
Cl	19·24		17·93	
	100·00			

The portion not distilled contained doubtless a trace of free benzoic acid, which would affect the carbon and chlorine, but not the hydrogen.

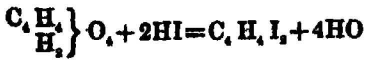
Chlor-benzoate of glycol, as I shall call this compound, has a pungent and somewhat bitter taste. It is insoluble in water, but freely soluble in alcohol and ether. Boiling solution of potash effects its decomposition with difficulty, solid potash readily, the reaction being the same as in the case of the analogous compounds.

Action of Hydriodic Acid on Glycol.—Formation of Iodide of Ethylene on Iodhydrine of Glycol.

Hydriodic acid gas is absorbed with great energy by glycol. A considerable quantity of heat is evolved during the passage of the gas, and the liquor becomes black and thick from the separation of free iodine. On removing the iodine by means of dilute potash, a mass of small white crystals is brought to light, which I at once suspected to be iodide of ethylene. To remove all doubt on this point, I submitted the crystals to analysis, having previously purified them, by recrystallizing from boiling alcohol. The numbers obtained agree with the formula of iodide of ethylene:—

	Theory.	Experiment.
C_4....	8·51	8·73
H_4..	1·42	1·78
I_2 . .	90·07	
	100·00	

The reaction which causes the formation of iodide of ethylene may be thus explained:—



$$\left.\begin{matrix} C_4H_4 \\ H_2 \end{matrix}\right\} O_4 + 2HI = C_4H_4I_2 + 4HO$$

That the action of hydriodic acid on glycol should be different from that of hydrochloric acid is doubtless owing to the bond of union between hydrogen and iodine being much weaker than that between hydrogen and chlorine

If, on the other hand, the temperature of the glycol be prevented from rising during the passage of the hydriodic acid gas, by surrounding the vessel containing it with cold water, a liquid product is obtained, which is coloured dark-brown by free iodine This I have not as yet been able to discover any means of purifying, it being soluble in water, and decomposed by distillation I believe, however, it is the compound corresponding to chlorhydrine of glycol $\left(\left.\begin{matrix}C_4H_4\\H\\Cl\end{matrix}\right\}O_2\right)$ discovered by M Wurtz A portion of this liquid, from which I had simply removed the free iodine, by agitation with mercury, gave, on analysis, numbers agreeing tolerably well with the formula of iodhydrine After the analysis, however, I discovered that it contained a considerable quantity of iodide of mercury in solution Another portion, from which I had removed the iodine by means of metallic silver, gave, on analysis, 11 1 per cent carbon and 3 5 hydrogen, instead of 13 9 carbon and 3 0 hydrogen. After all, an analysis is not necessary to enable us to arrive at the composition of this body. The products formed by the action of potash on it furnish us with almost as convincing a proof of its composition as any analysis could do They are iodide of potassium and oxide of ethylene

Iodhydrine of glycol is soluble in water and alcohol, but insoluble in ether. It has no taste at first, after a time, however, it almost burns the tongue, it is so pungent. It is decomposed by heat into iodide of ethylene, and probably glycol It acts with great energy on the salts of silver.

Action of Hydriodic and Acetic Acids on Glycol.—Formation of Iodacetine of Glycol

A stream of hydriodic acid gas was conducted into a mixture of equivalent quantities of glacial acetic acid and glycol, the temperature of which was prevented from rising during the action of the gas. As soon as a portion of the liquid gave a considerable quantity of an oily precipitate on the addition of water, the passage of the gas was interrupted, for the prolonged action of the gas is apt to give rise to the formation of iodide of ethylene The liquid thus obtained was well washed with very dilute potash, dried in vacuo, and analysed The numbers obtained lead to the formula $\underset{I}{\left.\begin{matrix}C_4H_4\\C_4H_3O_2\end{matrix}\right\}}O_2$, as will be seen from the following table —

	Theory	Experiment	
		I	II
C_8	22 42	21 95	22·30
H_7.	3 27	3 31	3 50
O_4	14·96		
I	59 35		
	100 00		

Iodacetine has a sweetish pungent taste It is insoluble in water, but soluble in alcohol and ether Its specific gravity is greater than that of water It crystallizes in tables when exposed to cold Heated with potash, it gives iodide of potassium, acetate of potash, and oxide of ethylene It is readily decomposed by the salts of silver

This compound can also be prepared with great facility by exposing monoacetate of glycol to the action of hydriodic acid gas The liquid must be kept cold during the action of the gas, which should be interrupted as soon as the addition of water to a portion of it causes an abundant oily precipitate The whole is then washed with dilute potash, and dried in vacuo A specimen prepared in this manner gave, on analysis, 22 62 per cent carbon and 3·43 hydrogen, instead of 22 42 carbon and 3 27 hydrogen

I hope soon to have an opportunity of studying these iodine compounds more particularly

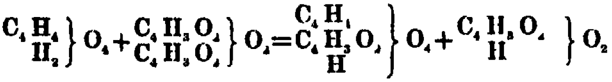
Action of Anhydrous Acetic Acid on Glycol.—Formation of Monoacetate of Glycol

A mixture of equivalent quantities of anhydrous acetic acid and glycol was heated in a sealed tube for several hours at a temperature not exceeding 170° C. On opening the tube, and submitting its contents to distillation, it was observed that the mercury remained stationary for a considerable time at about 120°, the point of ebullition of glacial acetic acid, and then rose rapidly to 180°, between which and 186° the remainder of the liquid passed over.

This was analysed, and proved to be pure monoacetate of glycol

	Theory	Experiment
C_8	46 15	46 02
H_8	7 69	7 80
O_6	46 16	
	100 00	

The following equation will explain the reaction which takes place between the acid and the glycol —



$$\left.\begin{matrix}C_4\,H_4\\H_2\end{matrix}\right\}O_4+\left.\begin{matrix}C_4\,H_3\,O_2\\C_4\,H_3\,O_2\end{matrix}\right\}O_2=\left.\begin{matrix}C_4\,H_4\\C_4\,H_3\,O_2\\H\end{matrix}\right\}O_4+\left.\begin{matrix}C_4\,H_3\,O_2\\H\end{matrix}\right\}O_2$$

The foregoing experiments were performed in the laboratory of M. Wurtz

VI "Experiments on some of the Various Circumstances influencing Cutaneous Absorption" By AUGUSTUS WALLER, M.D, F R S, Professor of Physiology, Queen's College, Birmingham. Received June 27, 1859

In some former experiments* I endeavoured to elucidate the phænomena of cutaneous absorption on the lower animals (batracia), by immersing the hinder extremities in various solutions, and afterwards watching the period at which the absorbed substances reached the tongue, where their presence was detected by means of some reagent applied to its surface, as, for instance, a salt of iron, when the legs were immersed in a solution of yellow ferro-cyanide of potassium, Prussian blue was then formed as soon as the ferro-cyanide was brought to the tongue

Furthermore, I was able to detect, by the aid of the microscope, the "lieux d'élection," or preference spots, where the cyanide escaped from the vessels.

On the present occasion I shall endeavour to elucidate cutaneous absorption on the higher animals, and, if possible, to give a more definite view of this function, by determining, by accurate measurement, the degree of rapidity, the peculiarities, &c., which it may offer in various conditions.

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* Waller "Absorption of various substances through the skin of the Frog."—*Frorieps Tagesberichte*, 1851.

A very simple mode of demonstrating the existence of cutaneous absorption is by immersing the leg of a young guinea pig, not more than half-grown, into a mixture of equal parts of chloroform and tincture of aconite After 15 minutes' immersion, the part will be found insensible at the surface and extremities, and, after a short time, symptons of poisoning by aconite will supervene, viz nausea, efforts at vomiting, sometimes vomiting of bile, coldness of the surface and extremities, circulation very weak, laborious respiration, slight convulsive symptoms, and death

The influence of age, or of thickening of the cuticle, is easily seen in the same way, for, if instead of a young animal we take an adult one, we obtain no poisoning, but merely local insensibility and slight disturbance of respiration, &c

Another not less instructive experiment consists in replacing the mixture of chloroform and tincture of aconite by simple tincture of aconite In this case, the limb may be indefinitely immersed without our obtaining either local insensibility, or death, or indeed any symptom whatever of the presence of aconite in the system

A fourth experiment, which consists in dividing the sciatic nerve, shows the influence of innervation on the function of absorption, for, if performed on an adult animal, and consequently one incapable of absorbing aconite in quantity sufficient to cause death, the powers of absorption will be generally found so much augmented that the animal will be poisoned by immersion of the limb in simple tincture of aconite

In this experiment I attribute the acceleration in the cutaneous absorption to the paralysis of the blood vessels, as in my experiments on the sympathetic nerve, where I showed that in blood vessels the passage of the blood is completely regulated by nerves springing from the spinal cord When the vascular nerves are paralysed, the artery becomes greatly distended, and the blood flows faster within it. The foot after the section of the sciatic is, on this account, more hot and red, and for the same reasons it is easy to account for the more rapid absorption of medicinal agents

A fifth experiment consists in placing a ligature on the limb, in order to impede the powers of absorption of the animal Although the ligature does produce this result, I was rather surprised to find how much less efficient it was than is generally represented; for, whenever the least symptoms of a toxic influence made their appearance, a ligature placed over the limb rarely succeeded in saving the animal

In order to obtain results more susceptible of measurement, I proceeded to substitute atropia for aconite, and to make use of the albino rat in lieu of the guinea pig By this means, I possessed an agent whose intervention was immediately detected by its action on the iris My choice of the albino rat was for the like reason, i e the facility which it offered for exact and easy measurement, in which respect this animal is far preferable to any other with which I am acquainted, unless we except the white mouse, which, however, is so liable to die from slight causes, that it is little adapted for most physiological experiments

The *modus operandi* which I generally adopt is to immerse the limb into a small 2-drachm bottle containing sufficient of the mixture to cover the foot and part of the leg The strength of the solution of atropia being generally that from half a grain to one drachm of some menstruum, such as chloroform, alcohol, &c , I generally prefer simply to hold the animal during the experiment to any other mode of restraint. By these means I am able to guard against several causes of error, such as the direct contact of the solution with the eye or mouth, and, at the same time, avoid any unnecessary discomfort to the animal

Chloroform and Atropia —A solution of atropia in chloroform will generally be found to cause dilatation of the pupil after the foot has been immersed from two to five minutes The dilatation, having once commenced, is usually very rapid, and the pupil very soon attains double or treble its normal diameter, which is about ¼ to ½ a millimetre during day-time It is easy to recognize that this dilatation is not in very simple ratio to the time occupied in its expansion, the expansion of the pupil being more nearly in proportion to the square of the time occupied than in a simple arithmetical ratio Immersion of one limb causes both pupils to dilate equally, except in some few instances, where one pupil expands much more than the other, from some constitutional peculiarity, which remains the same whichever foot be immersed

Although I have never failed to obtain dilatation of the pupils by the immersion of the foot in this solution of atropia, yet, in some cases, it takes place more slowly than in others The age of the animal has, in this respect, a most marked retarding influence On animals only about a third grown, it will often occur at about 2½ minutes after immersion, while in the adult it generally requires five minutes and upwards

The *local effects of immersion are redness, heat, and swelling of* the foot, accompanied sometimes with extravasation from some smaller vessels, when the immersion has been prolonged for ten minutes and upward The sensibility of the part is likewise diminished, but in no case so as to produce insensibility The amount of irritation is of course variable, according to the duration of the immersion It is, however, important to remark that full dilatation of the pupils may be obtained without any symptons beyond those of a temporary active vascularization of the part, which quickly disappears when the irritating cause is removed, and which presents no more active symptoms than those produced by neuro-paralysis of the vessels after section of the sciatic nerve

If instead of immersing the limb as above, we merely plunge it for a moment in the solution, we likewise may have dilatation of the pupil, but more slowly

The same effects are obtained even although the limb be washed on its withdrawal from the solution, which would lead to the inference that the effect in that case is owing to absorption of the atropia, at all events beneath the cuticle

In the case of a solution of atropia in turpentine, a still more curious effect is observed, viz that during immersion in the liquid the pupil scarcely, if at all, dilates, whereas, immediately after the removal of the limb, the dilatation commences Dilatation of the pupils will generally persist from twenty-four to thirty-six hours, and the return to the normal size is very gradual In some cases the pupil may be affected after an immersion of nine minutes, the dilatation reaching three millimetres, while in others, only a very slight influence is obtained on the pupil after an immersion of from twelve to fifteen minutes If the limb is then removed from the solution, the pupil dilates to its maximum in a few minutes. After two or three minutes' immersion, the animal shows signs of considerable pain Much inflammation of the part follows the action of this solution, which is followed by œdema

When we immerse the tail of the animal instead of its foot, absorption takes place much more slowly, dilatation of the pupil being produced only after the lapse of about twenty minutes.

Atropia and Alcohol.—If we substitute alcohol instead of chloroform as a solvent, we find that absorption is extremely slow Instead of obtaining dilatation of the pupils in two or three minutes, we find that an immersion of twenty to thirty minutes in the alcoholic solution will only produce very slight effects At the same time the local irritation is much less than that caused by chloroform Alcohol of various strength, from proof spirit upwards, had the same result as a solvent

Atropia in water, with the addition of sufficient acetic acid for its solution —The absorption of atropia in this state is very slow, thirty minutes' immersion frequently producing no dilatation of the pupils Dilatation is then promoted by removal of the limb from the solution

Watery extract of Belladonna —When rubbed over the leg and tail, this substance was not found, after the lapse of an hour, to produce any dilatation of the pupil

Tincture of belladonna, with half its quantity of chloroform, produced dilatation at the end of fifteen minutes The part was found on removal to be completely insensible, and considerably swollen from œdema, which lasted for several days

Atropia with strong alcohol and ammonia produced dilatation of the pupil after twenty-five minutes' immersion In this case, the ammonia was added for the purpose of ascertaining how far irritation of the part was conducive to absorption Slight vesication was the consequence of the presence of ammonia. The acceleration of the absorption was very slight, as the solution produced no dilatation until after twenty-four minutes' immersion

Absorption of Morphia.—The foot of a young rat at one-third of its growth was immersed in a solution consisting of half a grain of acetate of morphia in twenty drops of alcohol and one drachm of chloroform. In five minutes the pupils gradually dilated to the maximum; the limb was then withdrawn, foot hot, red, and rather swollen Irritation of the skin caused no cry, the animal merely withdrawing the part Somnolency existed, from which any noise aroused it, but only for a moment When placed on its back, the animal remained in that position Respiration accelerated. Vision when roused very imperfect, as was shown by its falling off the table.

The pupils continued fully dilated, the iris being reduced to an almost imperceptible circle, the dilatation exceeding that which I have been able to attain even with atropia. I will not dwell more fully at present on this last interesting fact, which is opposed to what we generally meet with in the administration of morphia Twelve hours after, pupils normal, animal quite well

Strychnia and Chloroform —After three minutes' immersion of foot, dilatation of pupils ensued After five minutes, the immersed limb was very sensitive, apparently more so than normal Limb removed from solution. spasms about the throat now appeared, which were rapidly succeeded by stiffness of the trunk, increasing into tetanic spasms Death, two minutes after removal.

Strychnia and Alcohol —Foot immersed in a solution of alcohol and strychnia for upwards of thirty-five minutes, no symptoms of strychnine poisoning Removed from solution and washed. Twelve hours later, no dilatation nor contraction of pupils.

The above observations evidently show that medicinal substances may be very rapidly absorbed into the circulation under certain circumstances, among which, the most important is the choice of the menstruum in which they are dissolved

It remains for us to examine into the effect of temperature, inflammation, neuro-vascular paralysis, &c , on absorption. But, what is of still more importance, we have to see how far these facts are applicable to man in health and disease

Meanwhile, I take this opportunity to state that a remarkable uniformity exists between cutaneous absorption in man and in the lower animals, and I believe that the application of these facts to practical medicine promises to be very important and extensive

VII. "On Spontaneous Evaporation" By BENJAMIN GUY BABINGTON, M.D , F.R.S., &c Received June 7, 1859

(Abstract)

The object of this communication is to make known certain powers of attraction and repulsion, hitherto, as far as I know, unnoticed, which are possessed by soluble substances in relation to their solvent, and which, in the case of water (the solvent here considered), are measured by the amount of loss, on spontaneous evaporation, in the

weight of solutions of different salts and other substances, as compared with the loss of weight in water

The force which holds together the particles of a vaporizable liquid is gradually overcome, if that liquid be exposed to air, by another force which separates, expands, and diffuses those particles in the form of vapour, and this separation takes place, even at a common temperature, so rapidly, provided the surface be sufficiently extensive, that an easy opportunity is afforded of determining the loss of weight by a common balance

A subject for investigation, possessing much interest, thus presents itself, and, in its pursuit, some new and unexpected results are encountered

The method which I have pursued has been to expose to the atmosphere, for a definite period, solutions of different salts, and also pure water under like conditions of quantity and area, temperature, atmospheric moisture, and atmospheric pressure

Different salts and other soluble substances are thus found to possess, when in solution, different powers of retarding or accelerating evaporation, and hence, from its amount, as compared with that which takes place in pure water, we can estimate the comparative value of those powers

The powers themselves being established as facts, the next point is to endeavour to discover the cause or causes on which they depend, and a wide field of inquiry is thus opened

The following are the instruments which have been employed .—

1 A balance, for one of the scales of which is substituted a flat metal plate, six inches square, on which the vessels to be weighed can be conveniently supported. This balance will turn sensibly at a grain, even with a weight of 4 lbs. on either side

2. A number of copper pans tinned within, all of the same size, being precisely 5 inches square inside, with perpendicular sides ¾ths of an inch in height, also a number of earthenware pans of the same dimensions, The area of 25 square inches has been chosen, partly because this size is convenient for manipulation, and partly because the results obtained can be easily represented in decimals. This facility of decimal calculation would be of importance should such pans come into general use as hygrometers, for which purpose they are well adapted.

3 Specific gravity bottles and counterpoises

4. Thermometers of various degrees of delicacy and range, for ascertaining freezing, temperate, and boiling points

5 Test tubes for use, in connexion with these thermometers, as well in freezing mixtures as over the spirit lamp.

6 A barometer

7 Various salts and other soluble substances, furnishing, when in solution, the materials for examination

The mode of procedure which I have adopted has been, to state my facts in the form of propositions, and to prove each of these propositions by experiments

The propositions are as follows ,—

1st proposition —That in many aqueous solutions of salts and other soluble substances evaporation is retarded, as compared with the evaporation of water

2nd proposition —That in solutions of salts which retard evaporation, that retardation is in proportion to the quantity of the salt held in solution.

3rd proposition.—That different salts and other substances soluble in water have different degrees of power in retarding its evaporation

4th proposition —That the power of retarding evaporation does not depend on the specific gravity of a solution

5th proposition —That in aqueous solutions of salts, the power of retardation does not depend on the base, whether we compare solutions containing like weights of the salt, or solutions of like specific gravities

6th proposition —That in aqueous solutions of salts, the power of retarding evaporation does appear to depend upon the salt radical or acid, although the retardation is not altogether independent of the influence of the base.

7th proposition.—That salts with two equivalents of an acid have a greater power of retarding evaporation than salts with one equivalent. There are, however, exceptions

8th proposition.—That there are some salts which, being dissolved in water, do not retard its evaporation, and some salts which, so far from retarding, actually accelerate evaporation.

The truth or probability of the foregoing propositions is established by numerous experiments, but in this abstract I shall, for the sake of brevity, only state the result of one or two experiments in proof of each.

The first proposition is proved by the fact that a solution of hydrochlorate of soda in the proportion of 480 grains to four measured ounces of water, when exposed under the conditions already stated to spontaneous evaporation, lost only 33 grains in weight after twelve hours' exposure,—while four ounces by measure of water lost 53 grains,—and after twelve hours' further exposure lost only 109 grains, while the water lost 174 grains, that is, the water, as compared with the solution, lost weight in the ratio nearly of 5 to 3

The second proposition is proved by the fact that a solution of 240 grains of hydrochlorate of soda in four ounces by measure of water lost in twelve hours 73 grains by evaporation, while four ounces by measure of pure water lost 81 grains,—this is in a proportion of only about 8 of the latter to 7 of the former, whereas, when double the quantity or 480 grains of salt were dissolved, the pure water, as compared with the solution, lost in the proportion of 5 to 3

The third proposition is proved by the fact that a solution of 480 grains of nitrate of potassa in 4 ounces or 1920 grains of water lost in twelve hours 95 grains, while a solution of the same strength of hydrochlorate of soda lost only 70 grains, and again, a solution of loaf-sugar, in which 480 grains were dissolved in 1920 grains of water, lost in 20 hours 175 grains, while a like solution of hydrochlorate of soda lost only 117 grains

The fourth proposition is proved by the fact that 480 grains of gum-arabic dissolved in 1920 of water had a specific gravity of 1 072, while a solution of hydrochlorate of soda of like strength had a specific gravity of 1·149, after 11¾ hours, the former had lost by evaporation 71 grains, while the latter had lost only 50 grains Here, therefore, the solution of the lighter specific gravity was *less* retarded in its evaporation than the heavier solution In contrast with this fact, a solution of hydrochlorate of ammonia of 480 grains to 1920 grains of water, having a specific gravity of only 1 060, lost by evaporation, in 8 hours and 44 minutes, 17 grains, while a like solution of hydrochlorate of soda lost 24 grains Here, then, the solution of lighter specific gravity was *more* retarded in its evaporation than the heavier solution. The conclusion is decisive that specific gravity has no necessary connexion with the phænomena

The fifth proposition is proved by the fact that in the following solutions of salts of potassa, all of the same strength (namely 1 salt to 10 water), a difference in the amount of evaporation in each will be observed to have taken place, and it must be borne in mind that in solutions so weak we cannot expect that difference to be very great

The reason for employing weak solutions was the necessity for having all of the same strength, one in ten being the extent, to which the least soluble salt submitted to examination, namely, the sulphate of potassa, will, at a low temperature, dissolve

	grains
Acetate of potassa lost in 35 hours .	145
Bicarbonate of potassa lost in 35 hours	131
Carbonate of potassa lost in 35 hours	115
Ferro-cyanate of potassa lost in 35 hours	110
Hydrochlorate of potassa lost in 35 hours	98
Nitrate of potassa lost in 35 hours	117
Sulphate of potassa lost in 35 hours	132
Tartrate of potassa lost in 35 hours	151

The above solutions were next made all of one specific gravity, namely 1 060, temp 62° Fahr , instead of being all of one strength, and the following is the result —

	grains
Acetate of potassa lost in 16½ hours	46
Bicarbonate of potassa lost in 16½ hours	45
Carbonate of potassa lost in 16½ hours	35
Ferro-cyanate of potassa lost in 16½ hours	41
Hydrochlorate of potassa lost in 16½ hours	32
Nitrate of potassa lost in 16½ hours .	39
Sulphate of potassa lost in 16½ hours	42
Tartrate of potassa lost in 16½ hours .	43

The sixth proposition is rendered probable by the following experiment, in which solutions are employed of acetic, nitric, sulphuric, and hydrochloric acids, combined respectively with potassa, soda, and ammonia, in the proportion of 100 grains of the salt to 1000 grains of water. After the expiration of 10 hours and 20 minutes, the solution of the three acetates lost respectively, for the potassa salt 35 grs., for the soda salt 35 grs., and for the ammonia salt 28 grs. In the solutions of the three nitrates, the loss was respectively 24, 25 and 25. In the solutions of the three sulphates, the loss was 30 grs., 37 grs., and 29 grs respectively, while in the solutions of the hydrochlorates it was 17, 18, and 19 grains

The seventh proposition is proved by an experiment in which a solution of 100 grains of carbonate of potassa dissolved in 1000 grains of water is compared with a like solution of bicarbonate of potassa. In ten hours the solution of the carbonate lost 45 grains, while that of the bicarbonate lost only 36 grains In comparing like proportions and quantities of sulphate and bisulphate of potassa, the respective losses in 13 hours were, for the former 53 grains, for the latter 45 grains. Similar comparisons of the acetate and binacetate of ammonia, phosphate and biphosphate, sulphate and bisulphate of potassa, tartrate and bitartrate of soda show like results In the course of investigating this proposition it was remarked incidentally that in all the salts examined, with the single exception of carbonate and bicarbonate of soda, the bin-acid solution (the proportion by weight of salt to water being equal) is of less specific gravity than the mono-acid solution, though possessing a greater power of retarding evaporation.

The eighth proposition, which seems extraordinary and even paradoxical, is proved by an experiment in which *saturated* solutions of—1, ferro-cyanate of potassa, 2, bitartrate of potassa, 3, sulphate of copper, 4, chlorate of potassa, and 5, distilled water, were compared In 9 hours and 20 minutes, their losses by evaporation were respectively 34 grs , 38 grs , 34 grs , 29 grs , and 29 grs , where we perceive that in the chlorate of potassa solution there has occurred no retardation at all, while in the following experiment, in which 120 grains of each of the salts examined were dissolved in 1200 grains of water, namely,—1, solution of sulphate of copper, 2, solution of ferro-cyanate of potassa, 3, solution of carbonate of soda, and 4, distilled water, the number of grains lost by evaporation after 15½ hours' exposure were,—1, 120 grs., 2, 113 grs , 3, 106 grs., 4, 103 grs.

It is thus perceived that in all the three solutions a more rapid evaporation had taken place than in distilled water alone

One or two other propositions are in process of investigation.

The paper concludes with a table of the freezing-points, boiling-points, and specific gravities, as well of weak as of saturated solutions, of the salts which have been submitted to examination

VIII. "On the Application of the Calculus of Probabilities to the results of measures of the Position and Distance of Double Stars." By THE LORD WROTTESLEY, V.P.R.S., &c

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In a communication addressed to the Royal Society "On the results of Periodical Observations of the Positions and Distances of certain Double Stars," printed in the Philosophical Transactions for 1851, I took occasion to remark that the differences between mean results obtained on different evenings were greater in proportion than those of the separate or partial measures obtained on the same evening, which arise from chance errors of observation, and that this circumstance rendered the application of the Formulæ of the Calculus of Probabilities to the reduction of the observations embarrassing and difficult In other words, the differences between the mean positions and distances obtained on different nights were greater than would have been anticipated by one who had merely computed the probable error of a single measure in the usual manner from the data furnished by the sums of the squares of the partial differences from the mean

The observations made since 1851 fully confirm the anomaly in question It is probable, therefore, that there is some cause which modifies sensibly and in some unknown manner the results obtained It may be temperature acting on the micrometer screw, it may be the state of the atmosphere or the method of making the observation, but whatever it be, the observations show conclusively that such causes are sometimes in operation

For the purpose of obtaining some numerical expression, however imperfect, of the effect produced, I adopted the following method — I took the difference between two mean results of position obtained on two different nights, where not more than about two months intervened between the observations, and I ascertained also the mean of the probable errors of such positions as computed in the ordinary method In order that each star might be subjected to exactly the same treatment, I selected always the observations of the first two nights on which it was observed, except when the two consecutive means were obtained at too long an interval apart Now as the number of partial measures of angle obtained on each separate night very often did not exceed six, these probable errors are certainly not theoretically correct, or to be depended upon absolutely as a test of the accuracy of the observation, but it may perhaps be assumed that any errors arising from this cause will not materially affect the mean of a very great number of results.

It appeared then that the mean of 218 differences taken at hazard from among such as were most accessible, and from observations made by different observers, was 37′ 67, and that the corresponding mean of the means of all the probable errors was 13′ 29, that is to say, the latter is 35 27 per cent only of the value of the former. As some proof that the cause, whatever it may be, is not very variable in its operation, I may add that the first 110 differences, which were all obtained before the end of 1854, give these numbers, 37′ 79, 11′ 86, and 31 37 per cent respectively Again, the last 108 differences, which were all derived from the observations of one observer only, give 37′ 56, 14′ 75, and 39 27 per cent Of these 108 last differences, the first 50, taken from the middle epoch of all the observations, give 40′·06, 14′ 60, and 36 44 per cent, and the last 58 of the 108 give 35′ 40, 14′ 88, and 42 03 per cent There is, however, a circumstance which must be taken into account in making a comparison between the first 110 differences and mean probable errors, and the last 108 During the course of the observations from which the former were derived, it was the practice to take always 10 measures of each star on each night when possible, during the observations from which the latter were derived, 6 measures only were taken This would tend to make the differences less in the former case, and with respect to its effect on the probable errors, if we put F for the error of a single measure in each case, the probable errors in the former case should be less by a quantity $=0\ 0768 \times F$, for if we put C for the constant and P for the probable error, then we have $P=\frac{3\sqrt{C}}{\sqrt{45}}\times F$, where 6 measures are obtained, and $P=\frac{\sqrt{5C}}{\sqrt{45}}\times F$, where 10 are taken, and the difference between these values $= 0768\ F$

The facts above disclosed create the difficulties in applying the Calculus of Probabilities which have been before referred to.

In the first place, the partial measures obtained on each separate night are generally too few in number to eliminate the effects of one-sided chance errors

In the second place, it seems probable that some cause remains in action during a whole night, modifying the result, whose origin and law remain to be discovered, but which seems tolerably constant in its operation.

The observations of my Catalogue of double stars are drawing to a close, and it became extremely desirable that if there were any fault in the reductions or method of computing hitherto employed it should be speedily remedied, and the necessary corrections made, I therefore applied to the Astronomer Royal, stating the embarrassments arising from the above-mentioned causes, and requesting his opinion as to the best mode of proceeding The Astronomer Royal exhibited on this, as on all other occasions, where his aid has been solicited, the greatest readiness to give me the benefit of his extensive knowledge of all that appertains to Astronomical science

Mr Airy observed that if there were a constant cause of error on any night, no multiplication of observations on that night would tend to remove it, and in that case he knew of no mode of proceeding which would *quite* meet the difficulty but the adoption of the following formulæ

Assuming that all the observations are equally good, or can be made so by grouping discordant measures, let f be the probable error of a single observation, and e the probable value of the error of each night Let S_1, S_2, S_3, &c. represent the sums of the squares of the errors obtained in the usual manner from the observations on the first, second, third, &c nights respectively, and put n_1, n_2, &c for the number of observations obtained on each of those nights, then the observations of the first night give,

$$(n_1-1)f^2 = 4549 \times S_1\,,$$

those of the second,

$$(n_2-1)f^2 = 4549 \times S_2,$$

and so on and from the sum of all these equations f may be accurately determined

Then to find e, compare the mean result obtained from the observations on all the nights, i e the mean of all the means, with the separate means for separate evenings, then putting S for the sum of the squares of the errors found by such comparison, and m for the whole number of nights, we have

$$(m-1)\,e^2 = 4549 \times S,$$

which gives the value of e

If A be taken to represent any convenient constant, the combining weight for *each* of the first night's observations will be $\frac{A}{n_1 e^2 + f^2}$, for each of the second night's $\frac{A}{n_2 e^2 + f^2}$, and so on

Let P stand for the probable error of the final result, then

$$P = \frac{1}{\sqrt{\left(\frac{n_1}{n_1 e^2 + f^2} + \frac{n_2}{n_2 e^2 + f^2} + \frac{n_3}{n_3 e^2 + f^2} + \&c\right)}}$$

The probable error of the mean of the first night's observations $= \sqrt{\left(e^2 + \frac{f^2}{n_1}\right)}$, of the second $= \sqrt{\left(e^2 + \frac{f^2}{n_2}\right)}$, &c

Mr Airy, however, while remarking that the mode of proceeding above described is the only one which really meets the difficulties of the case, admits at the same time that it would not be expedient to use so elaborate a process in dealing with observations like those in question, in which the ordinary errors of observation are large in amount, and in which such extreme accuracy in the results is not obtainable as in some other cases to which the principles of the Calculus are applicable

He suggests therefore that all the observations of all the several nights should be combined together for the purpose of obtaining the probable error and weight of the final result, and this may be done in two different ways —First, by treating all the single measures of all the nights, as if they had been made on one and the same night, and obtaining the final result and its probable error and weight accordingly in the usual manner Secondly, by treating each group or set of 6 or 10 as a single observation

The only other method of proceeding is that above described as the correct one, but which has not been adopted, as being too cumbrous for the occasion. This will be designated as the Third Method

For the purpose of ascertaining the result of employing each of these three methods, I requested my assistant, Mr Morton, to observe three stars, selected from among those which present only average difficulties, a very great number of times, so that the measures should be sufficiently numerous to eliminate all one-sided errors

The observations of Position only have been used, but these have been dealt with in the three different methods above described, that is, the final result and its probable error and weight have been obtained by each of the three modes The results are here subjoined, and the errors and their squares are given in full as to two of the stars, together with the whole computation, and it is to be hoped that this may not only prove interesting to observers of double stars, but may throw some light on the curious mathematical question involved in the inquiry which is the subject of the above remarks

Among the stars selected as above mentioned for the trial of the three methods, was 2 Comæ Berenices, or Σ 1596 Now this star had been very frequently observed during the six years from 1843 to 1848, at the time of the Parallax investigation, to which reference has been already made The comparison then made between the mean of all the measures of position obtained and the value of the angle of position given by Struve, gave reason to believe either that the angle had not altered during a period of sixteen years, or at least that it had altered very little The observations of 1859 fully confirmed this opinion Rejecting from the observations of 1843–8 those made on two nights, when less than 6 measures were obtained, the result of 1859 differs only 8' from that of 1843–8 I was thus enabled, for the purposes of this inquiry, to treat these observations of 1843–8, 156 in number, as if they had all been made within an interval of time not greater than about two months. Now these observations had been made by three different observers, and while the results of separate nights were very discordant, the probable errors derived from the partial values of nights, the results of which differed greatly from the general mean, were as remarkably small, on the other hand, the observations of the same star in 1859, 215 in number, were by one observer only, and the results of different nights agree very closely The applications of the three methods to the early and late observations of this star therefore illustrate very strikingly the effect produced by discordancy in the values obtained on different nights, when the peculiarities of the object observed are eliminated. Thus the good observations of 1859 give the e^2 equal to 89′ only, while in those of 1843–8 the e^2 attains the great value of 8731′.

The values of f^2 given by the observations of the three stars accord very well, considering the different circumstances under which they were obtained. It will be seen also that little effect is produced on the mean result by using these different methods of reduction

In the account of the American Coast Survey of 1856, and at pages 307–8, will be found a formula by which the probable error is deduced from the differences from the mean alone, the probable error or $P=0\ 845347\,\frac{\Sigma\epsilon}{n\sqrt{n-1}}$, where ϵ represents the error of a single observation

I have tested this in the case of three stars in which n was equal to 6, 10, and 156, respectively, and the probable error deduced was a little greater in the first two cases, and a very little smaller in the last.

Computation of P by Method 1 for Σ 1596, 2 Com Ber

Epoch	Sum of Measures	No of Meas.
1859 238	22° 29′	10
241	9 53	6
244	21 43	10
244	15 44	10
244	17 25	10
·244	20 10	10
·260	13 31	10
260	13 42	10
260	14 27	10
260	26 1	10
260	20 48	10
263	13 28	9
288	16 45	10
288	22 24	10
290	15 15	10
293	11 40	10
293	18 4	10
293	21 49	10
296	12 1	10
296	16 26	10
296	24 30	10
296	21 35	10
22)1 547	389 50	−215
	= 1 49	
1859 270	+ 30 0	
	31 49	
Zero	−450 19	
	418 30 238° 30′ = Mean Position	

Errors	e^2	Errors	e^2.	Errors	e^2	Errors	e^2
36	1296	82	6724	32	1024	53	2809
75	5625	26	676	40	1600	137	18769
39	1521	115	13225	21	441	22	484
42	1764	59	3481	48	2304	18	324
46	2116	15	225	63	3969	19	361
8	64	19	361	23	529	20	400
42	1764	12	144	6	36	2	4
9	81	29	841	29	841	65	4225
76	5776	167	27889	61	3721	58	3360
42	1764	34	1156	83	6889	3	9
58	3364	33	1089	14	196	20	400
23	529	53	2809	33	1089	9	81
54	2916	68	4624	14	196	33	1089
35	1225	94	8836	31	961	86	7396
12	144	68	4624	71	5041	79	6241
5	25	25	625	88	7744	33	1089
151	22801	35	1225	102	10404	100	10000
89	7921	3	9	69	4761	104	10816
61	3721	13	169	63	3969	35	1225
32	1024	13	169	21	441	60	3600
47	2209	78	6084	40	1600	10	100
34	1156	13	169	29	841	8	64
47	2209	39	1521	5	25	61	3721
67	4489	126	15876	17	289	57	3249
84	7056	78	6084	10	100	17	289
5	25	58	3364	32	1024	58	3364
19	361	79	6241	102	10404	7	49
3	9	64	4096	48	2304	17	289
49	2401	100	10000	48	2304	20	400
83	6889	37	1369	5	25	95	9025
41	1681	35	1225	134	17956	23	529
117	13689	94	8836	1	1	74	5476
5	25	39	1521	14	196	134	17956
35	1225	34	1156	21	441	91	8281
0	0	25	625	54	2916	82	6724
68	4624	85	7225	35	1225	41	1681
56	3136	113	12769	27	729	61	3721
14	196	26	676	86	7396	24	576
61	3721	48	2304	54	2916	51	2601
109	11881	105	11025	65	4225	27	729
128	16384	20	400	45	2025	127	16129
32	1024	89	7921	52	2704	8	64
181	32761	54	2916	31	961	92	8464
1	1	121	14641	28	784	56	3136
12	144	44	1936	85	7225	16	256
77	5929	131	17161	34	1156	109	11881
16	256	31	961	98	9604	204	41616
94	8836	87	7569	17	289	149	22201
10	100	73	5329	0	0	103	10609
149	22201	74	5476	94	8836	145	21025
76	5776	58	3364	34	1156		
68	4624	69	4761	47	2209		276891
92	8464	58	3364	25	625		152076
23	529	93	8649	23	529		266740
50	2500	35	1225	30	900		241952
	241952		266740		152076	S e^2 =	937659

Method 1 (*continued*)

Log of 937659 = 5 9720449
Constant = $\bar{6}$ 9950980

$P^2 = 9' 271$ = 0 9671429
$P = 3' 045$ = 0 4835715

W = 1079

Log of 215 = 2 3324385
Log of 214 = 2 3304138

4 6628523

Log of 454936 = $\bar{1}$ 6579503
4 6628523

Constant = $\bar{6}$ 9950980

Method 2

Mean Position = 238 30′

Errors	e^2	Errors	e^2
26	676	19	361
10	100	8	64
21	441	25	625
15	225	17	289
4	16	39	1521
12	144	1	1
28	784	22	484
27	729	37	1369
22	484	10	100
47	2209	38	1444
16	256	21	441
	6064		6699 6064
		$\Sigma e^2 =$	12763

Log of 12763 = 4 10595
$n = 22$, Constant = 4 99331

1 09926 = λ of P^2
0 54963 = λ of P

$P^2 = 12' 568$
$P = 3' 545$
W = ·0796

Method 3

$15063 = S_1$ $9f^2 = CS_1$
$7583 = S_2$ $5f^2 = CS_2$
$218206 = S_3$ $39f^2 = CS_3$
$241621 = S_4$ $49f^2 = CS_4$
$24012 = S_5$ $8f^2 = CS_5$
$48249 = S_6$ $19f^2 = CS_6$
$34511 = S_7$ $9f^2 = CS_7$
$85199 = S_8$ $29f^2 = CS_8$
$245830 = S_9$ $39f^2 = CS_9$

920274 $206 f^2 = C \times (S_1 + S_2 + \&c)$

Log. of 920274 = 5 9639172
Log of C = 4549 &c = 1 6579503

5·6218675
Log of 206 = 2 3138672

$f^2 = 2032' 4$ = 3 3080003

Method 3 (*continued*)

$b_1 = 32'\ 15'$	$b_1 - b = 26$ & $(b_1 - b)^2 = 676$	
$b_2 = 31\ 39$	$b_2 - b = 10$	100
$b_3 = 31\ 53$	$b_3 - b = 4$	16
$b_4 = 31\ 46$	$b_4 - b = 3$	9
$b_5 = 31\ 30$	$b_5 - b = 19$	361
$b_6 = 31\ 57$	$b_6 - b = 8$	64
$b_7 = 31\ 32$	$b_7 - b = 17$	289
$b_8 = 31\ 43$	$b_8 - b = 6$.	36
$b_9 = 31\ 52$	$b_9 - b = 3$	9
$b = 31\ 49$		$\Sigma = 1560$

See Method 1

Log of 1560 $= \underline{3}\ 1931246$
Constant Log $= \bar{1}\ 6579503$

$2\ 8510749$
Log of $8 = m - 1 = 0\ 9030900$

$e^2 = 88'\ 71$ $= 1\ 9479849$

$$P = \frac{1}{\sqrt{\left\{\left(\frac{10}{10\times 88\ 7 + 2032}\right) + \left(\frac{6}{6\times 88\ 7 + 2032}\right) + \left(\frac{40}{40\times 88\ 7 + 2032}\right)\right.}}$$

$$\overline{+\left(\frac{50}{50\times 88\ 7 + 2032}\right) + \left(\frac{9}{9\times 88\ 7 + 2032}\right) + \left(\frac{20}{20\times 88\ 7 + 2032}\right)}$$

$$\overline{\left. + \left(\frac{10}{10\times 88\ 7 + 2032}\right) + \left(\frac{30}{30\times 88\ 7 + 2032}\right) + \left(\frac{40}{40\times 88\ 7 + 2032}\right)\right\}}$$

$10\times 88\ 7 = 887$
$+ 2032$ $\lambda\ 10 = 1$

2919 & λ $= 3\ 46523$

1st W $= 003426$ $= \bar{3}\ 53477$

$6\times 88\ 7 = 532\ 2$
$+ 2032$ $\lambda\ 6 = 0\ 77815$

$2564\ 2$ & λ $= 3{\cdot}40895$

2nd W $= 002340$ $= \bar{3}\ 36920$

$40\times 88\ 7 = 3548$
$+ 2032$ $\lambda\ 40 = 1{\cdot}60206$

5580 & λ $= 3{\cdot}74663$

3rd W $= 007169$ $= \bar{3}\ 85543$

$50\times 88\ 7 = 4435$
$+ 2032$ $\lambda\ 50 = 1\ 69897$

6467 & λ $= 3{\cdot}81070$

4th W $= {\cdot}007732$ $= \bar{3}\ 88827$

$9\times 88\ 7 = 798\ 3$
$+ 2032{\cdot}$ $\lambda\ 9 = 0\ 95424$

$2830\ 3$ & λ $= 3\ 45184$

5th W $= {\cdot}003180$ $= \bar{3}\ 50240$

$20\times 88\ 7 = 1774$
$+ 2032$ $\lambda\ 20 = 1{\cdot}30103$

3806 & λ $= 3\ 58047$

6th W $= 005255$ $= \bar{3}\ 72056$

Method 3 (*continued*)

7th W = 003426

9th W = 007169

30 × 88 7 = 2661
+2032
4693

8th W = 006393

λ 30 = 1 47712
& λ = 3 67145
= $\bar{3}$ 80567

1st = ·003426
2nd = ·002340
3rd = 007169
4th = 007732
5th = 003180
6th = ·005255
7th = 003426
8th = 006393
9th = 007169

W = 046090

λ of 1 = 0
λ of 04609 = $\bar{2}$ 66361

1 33639 = λ of P^2
0 66820 = λ of P

P^2 = 21′ 70
P = 4′ 658
W = 0461

31°+

$a_1=75$ $W_1=34$
$a_2=39$ $W_2=23$
$a_3=53$ $W_3=72$
$a_4=46$ $W_4=77$
$a_5=30$ $W_5=32$
$a_6=57$ $W_6=53$
$a_7=32$ $W_7=34$
$a_8=43$ $W_8=64$
$a_9=52$ $W_9=72$

461

75
34
300
225
2550

39
23
117
78
897

53
72
106
371
3816

46
77
322
322
3542

30
32
60
90
960

57
53
171
285
3021

32
34
128
96
1088

43
64
172
258
2752

52
72
104
364
3744

2550
897
3816
3542
960
3021
1088
2752
3744

461)22370(48 5
1844
3930
3688
2420
2305

Mean result = 31 49
Zero = 450 19
418 30
Mean Angle = 238′ 30′

Σ 3049, σ Cassiopeæ   Computation of P   Method 1

Epoch	Sum	No of Meas
1858 572	32° 48′	6
712	32 54	6
821	64 27	10
857	64 13	10
857	60 25	10
876	56 9	10
876	50 43	10
876	63 0	10
887	53 33	10
887	53 0	10
890	56 58	10
890	63 2	10
12)10 001	651 12	−112
	= 5 49	
1858 833	+120 0	
	125 49	
Zero	=450 15	
	324° 26′ = Mean Position	

Errors	e²	Errors	e²	Errors	e²
132	17424	145	21025	14	196
18	324	31	961	159	25281
35	1225	16	256	10	100
30	900	9	81	1	1
66	4356	11	121	77	5929
47	2209	22	484	21	441
21	441	38	1444	32	1024
77	5929	56	3136	41	1681
163	26569	28	784	143	20449
89	7921	17	289	33	1089
35	1225	55	3025	5	25
17	289	45	2025	42	1764
7	49	39	1521	84	7056
21	441	26	676	85	7225
171	29241	97	9409	89	7921
63	3969	24	576	152	23104
54	2916	54	2916	54	2916
18	324	120	14400	115	13225
201	40401	95	9025	61	3721
75	5625	103	10609	153	23409
46	2116	78	6084	66	4356
97	9409	32	1024	67	4489
119	14161	89	7921	53	2809
64	4096	35	1225	6	36
56	3136	49	2401	61	3721
41	1681	72	5184	37	1369
14	196	86	7396	19	361
73	5329	15	225	30	900
105	11025	36	1296	4	16
41	1681	81	6561	28	784
32	1024	69	4761	106	11236
148	21904	5	25	75	5625
110	12100	97	9409	11	121
101	10201	9	81	37	1369
40	1600	46	2116	95	9025
73	5329	131	17161	52	2704
103	10609			65	4225
				87	7569
				16	256
	267375		155633		
					207528
					155633
					267375
				S e² =	630536

Log of 112 = 2 0492180
Log of 111 = 2 0453230

4 0945410

Log of 454936 = $\bar{1}$ 6579503
4 0945410

Constant = $\bar{5}$ 5634093

Log of 630536 = 5 7997099
Constant = 5 5634093

1 3631192
0 6815596

P² = 23 074
P = 4′ 8035
W = 0433

Method 2

°	′
324	47
324	46
323	48
323	50
324	11
324	38
325	11
323	57
324	54
324	57
324	33
323	57
12) 53	29
Mean = 324	27

Errors	e^2
20	400
19	361
39	1521
37	1369
16	256
11	121
44	1936
30	900
27	729
30	900
6	36
30	900
Σe^2	= 9429

Log of 9429 = 3 97447
Constant = $\bar{3}$ 53738
1 51185
0 75593

$P^2 = 32' \, 50$
$P = 5' \, 701$
$W = 0308$

Log 12 = 1 07918
Log 11 = 1 04139
2 12057
Log 4549 &c = $\bar{1}$ 65795
2 12057
Constant = $\bar{3}$ 53738

Method 3

$23792 = S_1$
$39974 = S_2$
$80279 = S_3$
$114116 = S_4$
$113454 = S_5$
$123083 = S_6$
$81960 = S_7$
576658

$5 f^2 = CS_1$
$5 f^2 = CS_2$
$9 f^2 = CS_3$
$19 f^2 = CS_4$
$29 f^2 = CS_5$
$19 f^2 = CS_6$
$19 f^2 = CS_7$
$105 f^2 = C \times (S_1 + S_2 + \&c)$

Log of 576658 = 5 7609183
Log of 4549 &c = 1 6579503
5 4188686
Log of 105 = 2 0211893
$f^2 = 2498' \, 5$ = 3 3976793

Method 3 (*continued*).

$b_1 = 125^{c}\ 28'$ $b_1 - b = 21$ & $(b_1 - b)^2 = 441$
$b_2 = 125\ 29$ $b_2 - b = 20$ .. 400
$b_3 = 126\ 27$ $b_3 - b = 38$ 1444
$b_4 = 126\ 14$ $b_4 - b = 25$ 625
$b_5 = 125\ 40$ $b_5 - b = 9$ 81
$b_6 = 125\ 20$ $b_6 - b = 29$ 841
$b_7 = 126\ 0$ $b_7 - b = 11$ 121

$b = 125\ 49$ $\Sigma = 3953$

See Method 1

Log of 3953 $= \underline{3}\ 5969268$
Log of 4549 &c $= 1\ 6579503$

$3\ 2548771$
Log of $6 = m - 1 = 0\ 7781513$

$e^2 = 299'\ 73$ $= 2\ 4767258$

$$P = \frac{1}{\sqrt{\left\{\left(\frac{6}{6\times 299\ 73+2498\ 5}\right)+\left(\frac{6}{6\times 299\ 73+2498\ 5}\right)+\left(\frac{10}{10\times 299\ 73+2498\ 5}\right)+\left(\frac{20}{20\times 299\ 73+2498\ 5}\right)+\left(\frac{30}{30\times 299\ 73+2498\ 5}\right)+\left(\frac{20}{20\times 299\ 73+2498\ 5}\right)+\left(\frac{20}{20\times 299\ 73+2498\ 5}\right)\right\}}}$$

$6\times 299\ 73 = 1798\ 38$
$+2498\ 5$ $\lambda\ 6 = 0\ 77815$

$4296\ 88$ & $\lambda = 3\ 63315$

1st W $= 001396$ $= \bar{3}\ 14500$

2nd W $= 001396$

$10\times 299\ 73 = 2997\ 3$
$+2498\ 5$ $\lambda\ 10 = 1$

$5495\ 8$ & $\lambda = 3\ 74003$

3rd W $= 001820$ $= \bar{3}\ 25997$

$20\times 299\ 73 = 5994\ 6$
$+2498\ 5$ $\lambda\ 20 = 1\ 30103$

$8493'1$ & $\lambda = 3\ 92907$

4th W $= 002355$ $= \bar{3}\ 37196$

$30\times 299\ 73 = 8991\ 9$
$+2498\ 5$ $\lambda\ 30 = 1\ 47712$

$11490\ 4$ & $\lambda = 4\ 06034$

5th W $= 002611$ $= \bar{3}\ 41678$

6th W $= 002355$

7th W $= 002355$

Method 3 (*continued*)

1st = 001396
2nd = 001396
3rd = 001820
4th = 002355
5th = 002611
6th = 002355
7th = 002355

W = 014288

λ of 1 = 0
λ of 014288 = $\bar{2}$ 15497

1 84503 = λ of P²
0 92252 = λ of P

P² = 69′ 99
P = 8′ 366
W = 01429

$a_1 = 28$ $W_1 = 14$ 28 × 14 = 392
$a_2 = 29$ $W_2 = 14$ 29 × 14 = 406
$a_3 = 87$ $W_3 = 18$ 87 × 18 = 1566
$a_4 = 74$ $W_4 = 24$ 74 × 24 = 1776
$a_5 = 40$ $W_5 = 26$ 40 × 26 = 1040
$a_6 = 20$ $W_6 = 24$ 20 × 24 = 480
$a_7 = 60$ $W_7 = 24$ 60 × 24 = 1440

125° +

144

144) 7100(49 3
576

1340
1296

440
432

Mean result = 125 49′
Zero = 450 15

Mean Angle = 324′ 26′

Summary of Results

Name of Star	Mean Position	No of Measures	Sum of ϵ^2	e^2	f^2	P²	P	W	Method
Σ 796	61° 8′	60	271,370	′	′	34′ 88	5′ 91	29	1
	61 8	,,				53 97	7 35	19	2
	61 4	,,		310 1	2026 7	117 7	10 85	9	3
Σ 1596, 2 Com Ber Early observations 1843–8.	238 38	156	3,529,521			66 41	8 15	15	1
	238 36	,,				545 61	23 36	2	2
	238 36	,,		8731 0	2178 0	559 85	23 66	2	3
Σ 1596 *continued* Observations of 1859	238 30	215	937,659			9 27	3 05	108	1
	238 30	,,				12 57	3 55	80	2
	238 30	,,		88 7	2032 4	21 70	4 66	46	3
Σ 3049, σ Cassiopeæ	324 26	112	630,536			23 07	4 80	43	1
	324 27	,,				32 50	5 70	31	2
	324 26	,,		299 7	2498 5	69 99	8 37	14	3

November 17, 1859

Sir BENJAMIN C BRODIE, Bart, President, in the Chair

In accordance with the Statutes, the President gave notice of the ensuing Anniversary Meeting for the election of Council and Officers

Captain Douglas Galton, John Denis Macdonald, Esq, George Murray Humphry, Esq, and William Odling, Esq, were admitted into the Society

Mr C C Babington, Sir Henry Holland, Mr Thomas Webster, the Rev R Willis, and Col Yorke, having been nominated by the President, were elected Auditors of the Treasurer's Accounts on the part of the Society

The following communications were read —

I "On the frequent occurrence of Vegetable Parasites in the Hard Structures of Animals" By Professor A KOLLIKER, of Wurzburg (See p 95)

II "Researches on the Phosphorus Bases"—No VI Phosphammonium-Compounds" By A W HOFMANN, LL D, F R S (See p 100)

III "Notes of Researches on the Poly-Ammonias"—No VI New Derivatives of Phenylamine and Ethylamine By A W. HOFMANN, F.R S (See p 101)

IV. "On the Behaviour of the Aldehydes with Acids" By A GEUTHER, Esq, and R. CARTMELL, Esq (For Abstract, see p. 108)

V. "On the Action of Acids on Glycol" (Second Notice) By Dr MAXWELL SIMPSON (See p 114)

VI "Experiments on some of the Various Circumstances influencing Cutaneous Absorption" By AUGUSTUS WALLER, M D, F R S. (See p. 122)

VII "On the Application of the Calculus of Probabilities to the results of measures of the Position and Distance of Double Stars" By The LORD WROTTESLEY, V P R S (See p 133)

VIII "Report of Scientific Researches made during the late Arctic Voyage of the Yacht 'Fox,' in search of the Franklin Expedition" By Captain M'CLINTOCK, R.N. Communicated by General SABINE, R A , Treas & V.P R S Received September 23, 1859

SIR,—I have the honour to acquaint you for the information of the President and Council of the Royal Society, that my voyage has happily terminated, and that our exertions have met with as great a measure of success as the most sanguine amongst us could expect But as the general result of Lady Franklin's "Final Search" will doubtless be made public before this letter reaches you, my object is simply to acquaint you with the nature and extent of such observations of scientific interest as we have been enabled to make

My last communication, dated 5th May, 1858, informed you of the unfortunate circumstances which led to our first Arctic winter being spent in an ice-drift out of Baffin's Bay During the winter of 1858–59 we were frozen up in a secure anchorage in Brentford Bay, which I have named "Port Kennedy," it is in Latitude 72° 01′ N and Longitude 94° 15′ W.

Here a magnetic observatory was built, the instruments supplied to us set up, and hourly observations continued during the interval between autumn and spring travelling Fortunately I was able to carry with me a 9½-inch dip-circle upon my journey to the Great Fish River this spring, and embraced every opportunity of making observations with it, many of them in the immediate vicinity of the point of maximum inclination Indeed it gives me much satisfaction to state that I believe our entire series of magnetical observations will be found complete —in as far as we were provided with the necessary instruments and favoured with the opportunities of using them.

Meteorological records have been carefully kept throughout the voyage, and the "Weather Books" supplied by Admiral FitzRoy filled up, also, comparisons of temperatures shown by a black-bulb thermometer in the sun's rays, and by others suspended against black and white surfaces, with those of a thermometer in the shade, were frequently made during two years.

Dr Joseph Hooker having suggested that some observations should be made on the temperature of the soil at different depths, such were registered at short intervals throughout the winter and spring of 1858–59

Observations upon the amount of ozone present in the atmosphere were made during the winter and spring of 1857–58, and also for eleven months at Port Kennedy, 1858–59

Whenever opportunity offered, the polariscope, supplied at the instance of Professor Stokes, was applied to halos, &c, and the amount and plane of polarization noticed

The direction of the Aurora and its influence on an electroscope, together with the periods of maximum and minimum intensity of atmospheric electricity, were observed

The periods of maximum and minimum barometric pressure were recorded, as deduced from hourly and two-hourly observations

A series of experiments was made on the change produced in sea-water by congelation at different temperatures

Deep-sea temperatures and specific gravities were taken when opportunity offered, also of the surface of the sea constantly

The great comet was seen at Port Kennedy, and a few angular measurements taken for determining its change of position, at intervals between 13th September and 8th October

Selections of native plants, from Port Kennedy and from Disco, have been brought home in Wardian cases, for the Royal Botanic Gardens at Kew Considerable collections have also been made in the various branches of Natural History, and Geological specimens from the lands visited have been brought home for scientific friends of the Expedition, who will speedily make public any interesting results. for these collections, and also for many of the observations made during the voyage, I am chiefly indebted to the Surgeon, David Walker, M.D.

A series of Tidal observations was taken at Port Kennedy, these will be discussed by the Rev. Professor Haughton, F.R.S., of Trinity College, Dublin.

Our geographical discoveries amount to nearly 800 miles of coast-line, they are interesting not only in consequence of their extent and the important position they occupy, but also from the great difficulty of access, whether by sea or land, to this newly explored area. With the exception of a comparatively small and unimportant part of the shore of Victoria Land, the whole of the coasts of Arctic America are now accurately delineated.

My sledge journey to the Magnetic Pole in February completed the discovery of the coastline of the American Continent The insularity of Prince of Wales Land was ascertained, and the discovery of its coastline completed, by a sledge party under the direction of the Sailing-master, Captain Allen Young, as also the west coast of North Somerset between Bellot Strait and Four River Bay Lieutenant Hobson, R N and his sledge party completed the discovery of the west coast of King William's Island, picking up the Franklin records, whilst with my own I explored its eastern and southern shores, returning northward by its west shore from the Great Fish River

Repeated attempts were made last year before the close of the navigable season, to reach open water visible in the broad channel westward of North Somerset, but a narrow barrier of ice across the western outlet of Bellot Strait, and there hemmed in so firmly by numerous islets as to continue unbroken throughout the autumn gales, foiled my sanguine hope of carrying the 'Fox,' according to my original plan, southward to the Great Fish River, passing east of King William's Island and from thence to some wintering position upon Victoria Land From a very careful scrutiny of the ice during my journeys over it in February, March, April, May, and June, it was evident that in this western sea it was all broken up, whilst eastward and southward of King William's Island there was hardly any ice last autumn, and therefore in all probability we *saw*, in that barrier of ice some three or four miles wide, the only obstruction to our complete success

The wide channel between Prince of Wales Land and Victoria Land, upon which I have conferred the name of Lady Franklin, admits a vast and continuous stream of very heavy ocean-formed ice from the north-west, which presses upon the western face of King William's Island and chokes up Victoria Strait.

I cannot divest myself of the belief that had Sir John Franklin been aware of the existence of a channel eastward of King William's *Land* (so named until 1854), and sheltered from this impenetrable ice-stream, his ships would safely and speedily have passed through it in 1846, and from thence with comparative ease to Behring Strait

Having enumerated the different subjects which have engaged the attention of the officers and myself and have employed much of our time, it only remains for me to express a hope that these will be found to be in some measure a justification of any moderate expectations which the President and Council of the Royal Society may have formed at the time of my departure from England in 1857, or at least to afford proof that my desire to be rendered useful in the advancement of science has in no degree abated since then

I am, Sir, your obedient Servant,

F L McClintock,

To the Secretary of the Royal Society *Captain R N*

November 24, 1859

Major-General SABINE, R A, Treasurer and V P, in the Chair

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows —

President —Sir Benjamin Collins Brodie, Bart, D C L

Treasurer.—Major-General Edward Sabine, R.A, D C L

Secretaries — William Sharpey, M D
George Gabriel Stokes, Esq, M A, D C L

Foreign Secretary —William Hallows Miller, Esq, M A

Other Members of the Council —C Cardale Babington, Esq, M.A, Rear-Admiral Sir George Back, D C L, Rev John Barlow, M A, Thomas Bell, Esq, Arthur Cayley, Esq, William Farr, M.D, D C.L, Sir H Holland, Bart, M D, D C L, Thomas Henry Huxley, Esq, Sir Roderick I Murchison, M A, D.C L., Thomas

Webster, Esq., M A ; Rev. William Whewell, D D , Alex. William Williamson, Ph D , Rev Robert Willis, M A , Sir William Page Wood, D C.L , The Lord Wrottesley, M A , Colonel Philip Yorke

Thomas Watson, M D , Frederick Crace Calvert, Esq , John Penn, Esq , and Lieut.-Col William Yolland, R E , were admitted into the Society

The following communications were read —

I. "On Recent Theories and Experiments regarding Ice at or near its Melting-point." By Professor JAMES THOMSON, Queen's College, Belfast Communicated by Professor WILLIAM THOMSON, F.R S Received September 9, 1859

My object in the following paper is to discuss briefly the bearings of some of the leading theories of the plasticity and other properties of ice at or near its melting-point, on speculations on the same subject advanced by myself*, and, especially, to offer an explanation of an experiment made by Professor James D Forbes, which to him and others has seemed to militate against the theory proposed by me, but which, in reality, I believe to be in perfect accordance with that theory

In the year 1850, Mr Faraday† invited attention, in a scientific point of view, to the fact that two pieces of moist ice, when placed in contact, will unite together, even when the surrounding temperature is such as to keep them in a thawing state He attributed this phenomenon to a property which he supposed ice to possess, of tending to solidify water in contact with it, and of tending more strongly to solidify a film or a particle of water when the water has ice in contact with it on both sides than when it has ice on only one side

In January 1857, Dr Tyndall, in a paper (by himself and Mr. Huxley) read before the Royal Society and in a lecture delivered at the Royal Institution, adopted this fact as the basis of a theory by which he proposed to explain the viscosity or plasticity of ice, or its capability of undergoing change of form, which was previously known to be the quality in glaciers in virtue of which their motion down their valleys is produced by gravitation Designating Mr. Faraday's fact under the term "regelation," Dr Tyndall described the capability of glacier ice to undergo changes of form, as being not true viscosity, but as being the result of vast numbers of successively occurring minute fractures, changes of position of the fractured parts, and regelations of those parts in their new positions The terms *fracture* and *regelation* then came to be the brief expression of his idea of the plasticity of ice He appears to have been led to deny the applicability of the term viscosity through the idea that the motion occurs by starts due to the sudden fractures *of parts in themselves not viscous or plastic* The crackling, he pointed out might, according to circumstances, be made up of separate starts distinctly sensible to the ear and to the touch, or might be so slight and so rapidly repeated as to melt almost into a musical tone He referred to slight irregular variations in the bending motion of the line marked by a row of pins on a glacier by Prof Forbes, as being an indication of the absence of any quality that could properly be called viscosity, and of the occurrence of successive fractures and sudden motions in a material not truly viscous or plastic I can only understand his statements on this subject by supposing that he conceived the material between the cracks to be rigid, or permanent in form, when existing under strains within the limit of its strength, or when strained less than to the point of fracture

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* Proceedings of Royal Society, May 1857 Also British Association Proceedings, Dublin Meeting, 1857 Also Proceedings of Belfast Literary and Philosophical Society, December 2, 1857

† Lecture by Mr Faraday at the Royal Institution, June 7, 1850 ; and Report of that Lecture, Athenæum, 1850, p 640.

This theory appeared to me to be wrong*, and I then published, in a paper communicated to the Royal Society, a theory which had occurred to me mainly in or about the year 1848, or perhaps 1850, but which, up till the date of the paper referred to, had only been described to a few friends verbally That theory of mine may be sketched in outline as follows —If to a mass of ice at its melting-point, pressures tending to change its form be applied, there will be a continual succession of pressures applied to particular parts—liquefaction occurring in those parts through the lowering of the melting-point by pressure—evolution of the cold by which the so melted portions had been held in the frozen state—dispersion of the water so produced in such directions as will afford relief to the pressure—and recongelation, by the cold previously evolved, of the water on its being relieved from this pressure and the cycle of operations will then begin again, for the parts re-congealed, after having been melted, must in their turn, through the yielding of other parts, receive pressures from the applied forces, thereby to be again liquefied and to proceed through successive operations as before

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* While the offering of my own theory as a substitute for Professor Tyndall's views seems the best argument I can adduce against them, still I would point to one special objection to his theory No matter how fragile, and no matter how much fractured a material may be, yet if its separate fractured parts be not possessed of some property of internal mobility, I cannot see how a succession of fractures is to be perpetuated. A heap of sand or broken glass will either continue standing, or will go down with sudden falls or slips, after which a position of repose will be attained, and I cannot see how the addition of a principle of reunion could tend to reiterate the fractures after such position of repose has been attained When these ideas are considered in connexion with the fact that while ice is capable of standing, without immediate fall, as the side of a precipitous crevasse, or of lying without instantaneous slipping on a steeply sloping part of a valley, it can also glide along, with its surface nearly level, or very slightly inclined, I think the improbability of the motion arising from a succession of fractures of a substance having its separate parts devoid of internal mobility will become very apparent If, on the other hand, any quality of internal mobility be allowed in the fragments between the cracks, a certain degree at least of plasticity or viscosity is assumed,

Professor Tyndall, in papers and lectures subsequent to the publication of this theory, appears to adopt it to some extent, and to endeavour to make its principles cooperate with the views he had previously founded on Mr Faraday's fact of so called "regelation"*

Professor James D Forbes adopts Person's view, that the dissolution of ice is a *gradual*, not a *sudden* process, and so far resembles the tardy liquefaction of fatty bodies or of the metals, which in melting pass through intermediate stages of softness or viscosity He thinks that ice must essentially be colder than water in contact with

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in order to explain the observed plasticity or viscosity That fractures—both large and exceedingly small—both large at rare intervals, and small, momentarily repeated—do, under various circumstances, arise in the plastic yielding of masses of ice, is, of course, an undoubted fact but it is one which I regard not as the cause, but as a consequence, of the plastic yielding of the mass in the manner supposed in my own theory It yields by its plasticity in some parts until other parts are overstrained and snap asunder, or perhaps also sometimes slide suddenly past one another

* I suppose the term regelation has been given by Prof Tyndall as denoting the second, or mending stage in his theory of "*fracture* and *regelation*" Congelation would seem to me the more proper word to use after fracture, as *regelation* implies previous melting If my theory of *melting by pressure and freezing again on relief of pressure* be admitted, then the term regelation will come to be quite suitable for a part of the process of the union of the two pieces of ice, though not for the whole, which then ought to be designated as the process of *melting* and *regelation*

it, that between the ice and the water there is a film varying in local temperature from side to side, which may be called plastic ice, or viscid water, and that through this film heat must be constantly passing from the water to the ice, and the ice must be wasting away, though the water be what is called *ice-cold*

There is a manifest difficulty in conceiving the possibility of the state of things here described and I cannot help thinking that Professor Forbes has been himself in some degree sensible of the difficulty, for in a note of later date by a few months than the paper itself, he amends the expression of his idea by a statement to the effect that if a small quantity of water be enclosed in a cavity in ice, it will undergo a gradual "*regelation*," that is, that the ice will in this case be gradually increased instead of wasted In reference to the first case, I would ask,—What becomes of the cold of the ice, supposing there to be no communication with external objects by which heat might be added to or taken from the water and ice jointly considered? Does it go into the water and produce viscidity beyond the limit of the assumed thin film of viscid water at the surface of the ice? Precisely a corresponding question may be put relatively to the second case—that of the large quantity of ice enclosing a small quantity of water in which the reverse process is assumed to occur Next, let an intermediate case be considered—that of a medium quantity of water in contact with a medium quantity of ice, and in which no heat, nor cold, practically speaking, is communicated to the water or the ice from surrounding objects This, it is to be observed, is no mere theoretical case, but a perfectly feasible one The result, evidently, if the previously described theories be correct, ought to be that the mixture of ice and water ought to pass into the state of uniform viscidity Prof Forbes's own words distinctly deny the permanence of the water and ice in contact in their two separate states, for he says, "bodies of different temperatures cannot continue so without interaction The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes even though the water be what is called ice-cold*" Now the conclusion arrived it, namely, that a quantity of viscid water could be produced in the manner described, is, I am satisfied, quite contrary to all experience No person has ever, by any peculiar application of heat to, or withdrawal of heat from, a quantity of water, rendered it visibly and tangibly viscid We even know that water may be cooled much below the ordinary freezing-point and yet remain fluid

Professor Forbes regards Mr Faraday's fact of regelation as being one which receives its proper explanation through his theory described above, and, in confirmation of the supposition that ice has a tendency to solidify a film of water in contact with it, and in opposition to the theory given by me, that the regelation is a consequence of the lowering of the melting-point in parts pressed together, he adduces an experiment made by himself, which I admit presents a strong appearance of proving the influence of the ice in solidifying the water, to be not essentially dependent on pressure This experiment, however, I propose to discuss and explain in the concluding part of the present paper

Professor Forbes accepts my theory of the plasticity of ice as being so far correct that it points to *some* of the causes which may reasonably be considered, under peculiar circumstances, to impart to a glacier a portion of its plasticity In the rapid alternations of pressure which take place in the moulding of ice under the Bramah's press, it cannot, he thinks, be doubted that the opinions of myself and my brother Professor Wm Thomson are verified*

Mr Faraday, in his recently published 'Researches in Chemistry and Physics,' still adheres to his original mode of accounting for the phenomenon he had observed, and for which he now adopts the name "regelation," or, at least, while alluding to the views of Prof Forbes as possibly being admissible as correct, and to the explanation offered by myself as being probably true in principle, and possibly having a correct bearing on the phenomena of regelation, he considers that the principle originally assumed by himself may after all be the sole cause of the effect. The principle he has in view, he then states as being, when more distinctly expressed, the following.—"In all uniform bodies possessing cohesion, *i e* being either in the liquid or the solid state, particles which are surrounded by other particles having the like state with themselves tend to preserve that state, even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so-surrounded, would cause them instantly to change their condition" Referring to water in illustration, he says that it may be cooled many degrees below 32° Fahr., and still retain its liquid state, yet that if a piece of the same chemical substance—ice—at a higher temperature be introduced, the cold water freezes and becomes warm He points out that it is certainly not the change of temperature which causes the freezing, for the ice introduced is warmer than the water, and he says he assumes that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and causes the change Exemplifying, in another direction, the principle he is propounding, he refers to the fact that water may be exalted to the temperature of 270° Fahr, at the ordinary pressure of the atmosphere, and yet remain water, but that the introduction of the smallest particle of air or steam will cause it to explode, and at the same time to fall in temperature He further alludes to numerous other substances—such as acetic acid, sulphur, phosphorus, alcohol, sulphuric acid, ether, and camphine—which manifest like phenomena at their freezing- or boiling-points, to those referred to as occurring with the substance of water, ice, and steam, and he adverts to the observed fact that the contact of extraneous substances with the particles of a fluid usually sets these particles free to change their state, in consequence, he says, of the cohesion between them and the fluid being imperfect, and he instances that glass will permit water to boil in contact with it at 212° Fahr, or by preparation can be made so that water will remain in contact with it at 270° Fahr without going off into steam, also that glass can be prepared so that water will remain in contact with it at 22° Fahr without solidification, but that an ordinary piece of glass will set the water off at once to freeze.

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* Forbes 'On the Recent Progress and Present Aspect of the Theory of Glaciers,' p 12 (being Introduction to a volume of Occasional Papers on the Theory of Glaciers), February 1859

He afterwards comes to a point in his reasoning which he admits may be considered as an assumption. It is "that many particles in a given state exert a greater sum of their peculiar cohesive force upon a given particle of the like substance in another state than few can do, and that as a consequence a water particle with ice on one side, and water on the other, is not so apt to become solid as with ice on both sides; also that a particle of ice at the surface of a mass [of ice] in water is not so apt to remain ice as when, being within the mass there is ice on all sides, temperature remaining the same."

This supposition evidently contains two very distinct hypotheses The former, which has to do with ice and water present together, I certainly do regard as an assumption, unsupported by any of the phenomena which Mr Faraday has adduced The other, which has to do with a particle of ice in the middle of continuous ice, and which assumes that it will not so readily change to water, as another particle of ice in contact with water, I think is to be accepted as probably true I think the general bearing of all the phenomena he has adduced is to show that the particles of a substance when existing all in one state only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience *a difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid but I do not think anything has been adduced showing a like difficulty as to their undergoing a change of state, when the substance is present in the two states already, or when a beginning of the change has already been made I think that when water and ice are present together, their freedom to change their state on the slightest addition or abstraction of heat, or the slightest change of pressure, is perfect I therefore cannot admit the validity of Mr Faraday's mode of accounting for the phenomena of regelation

Thus the fact of regelation which Prof Tyndall has taken as the basis of his theory for explaining the plasticity of ice, does in my opinion as much require explanation as does the plasticity of ice which it is applied to explain The two observed phenomena, namely the tendency of the separate pieces of ice to unite when in contact, and the plasticity of ice, are indeed, as I believe, cognate results of a common cause They do not explain one another They both require explanation, and that explanation, I consider, is the same for both, and is given by the theory I have myself offered

I now proceed to discuss the experiment by Prof Forbes, already referred to as having been adduced in opposition to my theory He states that mere *contact* without pressure is sufficient to produce the union of two pieces of moist ice *, and then states, as follows, his experiment by which he supposes that this is proved —"Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical Contact of the even surfaces was obtained by means of two very weak pieces of watch spring In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of twenty or more square inches) continued united In fact it appeared as complete as in another experiment where similar surfaces were pressed together by weights " He concludes that the effect of pressure in assisting 'regelation' is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another

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* "On some Properties of Ice near its Melting-Point," by Prof Forbes, Proceedings Royal Soc. Edin, April 1858

I have myself repeated this experiment, and have found the results just described to be fully verified It was not even necessary to apply the weak pieces of watch-spring, as I found that the pieces of ice, on being merely suspended on the glass rod in contact, would unite themselves strongly in a few hours Now this fact I explain by the capillary forces of the film of interposed water as follows —Firstly, the film of water between the two slabs—being held up against gravity by the capillary tension, or contractile force, of its free upper surface, and being distended besides, against the atmospheric pressure, by the same contractile force of its free surface round its whole perimeter, except for a very small space at bottom, from which water trickles away, or is on the point of trickling away—exists under a pressure which, though increasing from above downwards, is everywhere, except at that little space at bottom, less than the atmospheric pressure Hence the two slabs are urged towards one another by the excess of the external atmospheric pressure above the internal water pressure, and are thus pressed against one another at their places of contact by a force quite notable in its amount If, for instance, between the two slabs there be a film of water of such size and form as might be represented by a film one inch square, with its upper and lower edges horizontal, and with water trickling from its lower edge, it is easy to show that the slabs will be pressed together by a force equal to the weight of half a cubic inch of water But so small a film as this would form itself even if the two surfaces of the ice were only very imperfectly fitted to one another If, again, by better fitting, a film be produced of such size and form as may be represented by a square film with its sides 4 inches each, the slabs will be urged together by a force equal to the weight of half a cube of water, of which the side is 4 inches, that is, the weight of 32 cubic inches of water or 1 15 pound, which is a very considerable force Secondly, the film of water existing, as it does, under less than atmospheric pressure, has its freezing-point raised in virtue of the reduced pressure, and it would therefore freeze even at the temperature of the surrounding ice, namely the freezing-point for atmospheric pressure Much more will it freeze in virtue of the cold given out in the melting by pressure of the ice at the points of contact, where, from the first two causes named above, the two slabs are urged against one another

The freezing of ice to flannel or to a worsted glove on a warm hand is, I consider, to be attributed partly to capillary attraction acting in similar ways to those just described, but in many of the observed cases of this phenomenon there will also be direct pressures from the hand, or from the weight of the ice, or from other like causes, which will increase the rapidity of the moulding of the ice to the fibres of the wool

II. "On Spontaneous Evaporation" By Benjamin Guy Babington, M D, F R S &c. Received June 7, 1859. (For Abstract, see p 127)

November 30, 1859

ANNIVERSARY MEETING

Sir BENJAMIN C BRODIE, Bart., President, in the Chair

Colonel Yorke reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including £3466 3*s* 2*d.* received from the Stevenson bequest, amounted to £7016 0*s* 5*d*, and the total expenditure, including £2700 invested in the Funds, amounted to £6596 0*s.* 5*d.*, leaving a balance in the Treasurer's hands amounting to £420.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following lists —

Fellows deceased since the last Anniversary.

Henry Alexander, Esq
Richard Bright, M.D.
Frederick William, Marquis of Bristol
William John Broderip, Esq
Sir Arthur de Capell Brooke, Bt
Isambard Kingdom Brunel, Esq
Thomas Cook, Esq
William Nairn Forbes, Esq
Baron De Goldsmid
Sir Thomas Grant, K C B
Thomas Philip, Earl De Grey, K G
Thos , Earl of Haddington, K P
Henry Hallam, Esq , M A.
William Richard Hamilton, Esq
Arthur Henfrey, Esq
Thomas Hetherington Henry, Esq.
Thomas Horsfield, M D.
Manuel John Johnson, Esq., M.A.
Bishop Maltby, D D
Rev Charles Mayo, B D
George Moore, Esq
Frederick John, Earl of Ripon
Sir George T Staunton, Bart.
Robert Stephenson, Esq , D C L
Rev Henry Walter, B D
John Welsh, Esq.

On the Foreign List

Gustav Lejeune Dirichlet
Baron Alexander von Humboldt
Carl Ritter

Fellows elected since the last Anniversary

Samuel Husbands Beckles, Esq
Frederick Crace Calvert, Esq
Henry J. Carter, Esq
Capt Douglas Galton, R E.
William Bird Herapath, M D.
George Murray Humphry, Esq
Thomas Sterry Hunt, Esq
Archibald Campbell Tait, Lord Bishop of London.
John Denis Macdonald, Esq
William Odling, Esq.
Robert Patterson, Esq
John Penn, Esq
Robert Bickersteth, Lord Bishop of Ripon.
Sir Robert Schomburgk
Thomas Watson, M D
Bennet Woodcroft, Esq
Lieut.-Col. William Yolland, R.E

The President then addressed the Society as follows :—

GENTLEMEN,

IN an address lately delivered at a Meeting of the Society for the Promotion of Social Science, a noble Lord, a Fellow of this Society, called the attention of his hearers to the advantages which the world in general had derived from the cultivation of the physical sciences No one indeed can be better qualified to give an opinion on this subject than the distinguished individual to whom I have alluded His first communication to the Royal Society was in the year 1796, and was published in the 'Philosophical Transactions' for that year From that time to the present day he has, without any intermission, laboured for the advancement of all kinds of knowledge, and so he still continues to labour with all the determination and energy and intellectual vigour of youth, and I may confidently affirm that little has been done worthy of note during this interval of sixty-four years which has escaped his acute observation The influence, however, which the physical sciences have had in adding to the conveniences and comforts, and advancing the material prosperity of mankind is too obvious to escape the notice of a much less close observer than Lord Brougham If our houses and our cities are better and more economically lighted, if our population is better and more cheaply clothed, if our fields are more productive, if we travel by steam and communicate with those who are hundreds of miles distant from us by the telegraph, if a brighter light shines in our light-houses to guide the mariner at night, these and a thousand of things besides are but the result of the application by practical men, of the discoveries made in the physical sciences, to practical purposes To the same cause may be attributed much of the political greatness of the British nation The British flag floats in every sea, our colonies are established in every region of the earth, we contemplate in them with a reasonable pride the germs of future nations, which, when our fortune may possibly be changed, will speak the same language with ourselves, inheriting our literature, our political institutions, and not only our religion but our religious freedom, inheriting also our knowledge, and adding knowledge to it, but none of this could have been, if it were not that the astronomer had instructed the sailor how, with nothing but the heavens above him and the waters on every side, he may find his exact position on the surface of the globe .

But it would be a grave mistake to suppose that such as those which I have now enumerated are the only advantages which have been derived from the cultivation of the physical sciences. To know their full extent, we must take into the account not only the direct but also the indirect results to which it has led, and I trust that I may be excused if, on the occasion of the present anniversary, I occupy some portion of your time, not by an elaborate discussion of the subject, but by offering to you some suggestions as to the other ways in which inquiries such as those in which you are yourselves engaged have already affected, and may be expected still more to affect hereafter, the habits, the modes of thought, the fortunes and moral condition of mankind

It is not our business to depreciate that form of civilization which existed in times long since past, and especially of that remarkable people who during some centuries before and after the Christian æra were distinguished for their still unrivalled excellence in art, their noble literature, when Aristotle sat at the feet of his master Plato, when students in search of intellectual improvement from all parts of Greece resorted to the Lyceum of Athens, when from opposite quarters of the Mediterranean Sea the Greek colonies of Alexandria and Syracuse supplied a list of mathematicians and poets to add lustre to their parent state Neither let us forget what we owe to another people, whose civilization is to be measured, not by their wealth and luxury, their ambition and their conquests, but by those monuments of art which still draw visitors to Rome, their historians, moral philosophers, and poets But, great as are the obligations which we owe to these nations of antiquity, it cannot be denied that the civilization which exists among us at the present time is of a higher order than that which existed formerly and it is not difficult to show that it is to the greater extension of a knowledge of natural phenomena, and the laws which govern them, that this improvement is mainly to be attributed

Knowledge and wisdom are indeed not identical, and every man's experience must have taught him that there may be much knowledge with little wisdom, and much wisdom with little knowledge. But with imperfect knowledge it is difficult or impossible to arrive at right conclusions. Many of the vices, many of the miseries, many of the follies and absurdities by which human society has been infested and disgraced may be traced to a want of knowledge It was from a want of knowledge that Roger Bacon was persecuted by the Franciscan monks, and Galileo by the Inquisition, that Servetus was burned by Calvin, while others would have burned Calvin in his turn if they had had the opportunity of doing so So it was that juries were found to convict and judges to condemn poor ignorant women as witches, that within the last two centuries well-educated men believed that they might read their destiny in the stars, and that as lately as the year 1638, on the occasion of the birth of Louis XIV, Richelieu compelled the dungeons of the Inquisition to give up the astrologer Campanella, in order that he might cast the horoscope of the future king, and so it is that at the present day grown-up ladies and gentlemen occupy themselves with the humbler and less romantic mysteries of turning and rapping tables Cooperating with a purer religious faith, the advancement of knowledge has humanized our institutions It has banished slavery, it has caused our laws to be more merciful, and the administration of them more just, it has promoted religious and political freedom, and, with one or two miserable exceptions, it has rendered even despotic governments more attentive to the claims and wishes of their subjects If sanitary and other improvements (these being the results of greater knowledge) have added to the average length of human life, be it observed that this fact includes another fact, namely, that they have added to human happiness, for true it is that the causes which tend to the shortening of life are, with few exceptions, such as produce either physical pain or moral suffering

The investigation of the physical sciences is especially favourable to the training of some of the more important faculties of the mind, so that we may well anticipate much ultimate advantage from the movement which is already begun, having for its object, not to supersede the study of ancient languages and ancient literature (which at the present time, in addition to mathematics, are supposed to form the staple of a first-rate education), but to add an elementary knowledge of the principal physical sciences to the list The including of some of these at least in the instruction of early life will operate beneficially in various ways The first step in all physical investigations, even in those which admit of the application of mathematical reasoning and the deductive method afterwards, is the observation of natural phenomena, and the smallest error in such observation in the beginning is sufficient to vitiate the whole investigation afterwards The necessity of strict and minute observation, then, is the first thing which the student of the physical sciences has to learn, and it is easy to see with what great advantage the habit thus acquired may be carried into everything else afterwards Slovenly habits of observation are indeed the source of a large proportion of the evils which mankind bring upon themselves—of blunders in private life by which an individual causes the ruin of himself and his wife and children, of blunders in statesmanship which bring calamities on nations It is to these, moreover, that impostors and fanatics of all kinds and in all ages have been indebted for their influence and success

It would be easy to show how in various other ways the study of the physical sciences cannot fail to be a useful training for the mind Very much indeed might be said on this subject, but to enter fully into it would not only occupy too much of your time, but would involve us in a metaphysical discussion unsuited to the present occasion There are, nevertheless, two or three points to which I shall venture, however briefly, to allude

Investigations of this kind, more than almost any other, impress the mind with the necessity of looking carefully at both sides of a question, and strictly comparing the evidence on one side with that on the other, and in this manner they help to correct and improve the judgment As in every such investigation classification is an important and indeed a necessary element, another effect is that of promoting and strengthening the best kind of memory—a memory founded on some actual relation of objects to each other, and not on mere apparent resemblance and juxtaposition Lastly, physical investigations more than anything besides help to teach us the actual value and the right use of the imagination,—of that wondrous faculty which, left to ramble uncontrolled, leads us astray into a wilderness of perplexities and errors, a land of mists and shadows, but which, properly restrained by experience and reflection, becomes the noblest attribute of man—the source of the poetic genius, the instrument of discovery in science, without the aid of which Newton would never have invented fluxions, nor Davy have decomposed the earths and alkalies, nor would Columbus have found another continent beyond the Atlantic Ocean

In the pursuit of the physical sciences, the imagination supplies the hypothesis which bridges over the gulf that separates the known from the unknown It may be only a phantom, it may prove to be a reality But as these sciences relate to matters of fact which, if not directly, may be made indirectly cognizable by the external senses, they afford us peculiar facilities, far beyond what exist in other departments of knowledge, of testing the accuracy of the views which the imagination has suggested, so that we may at once determine when it has been too excursive, and, if it has been so, call it back to its right place There may be instances of mere accidental discovery, but, setting these aside, the great advances made in the inductive sciences are, for the most part, preceded by a more or less probable hypothesis The imagination, having some small light to guide it, goes first. Further observation, experiment, and reason follow Thus, for example, it had been long suspected that there is some sort of relation between electricity and magnetism Much thinking on the subject had strengthened this suspicion in the mind of Oersted Still it was but a hypothesis, and might even now have been regarded by many as no better than a dream, if it had not been that in the year 1820 the Danish philosopher devised the experiments which demonstrated the law of reciprocity between an electric current and the magnet, and the identity of the two forces As an instance of an opposite kind, I may refer to the doctrine of phlogiston as propounded by Stahl. While the art of chemical experiment was imperfectly understood, that doctrine was very generally received as affording a true explanation of the phenomena of combustion. But no sooner had Lavoisier and his friends introduced a more accurate mode of experiment by weight and measure, than it was proved to have no foundation in reality, and consigned to the same place in the history of science with epicycles and vortices and animal spirits

But the effect of some kind of instruction in the physical sciences being recognized as an essential part of a liberal education, may be contemplated under another point of view. Except in the case of particular professions or occupations, a profound knowledge of these subjects is not required, but there is no situation in life in which some knowledge of them may not be turned to a good account. Is there any country gentleman or farmer who might not derive advantage from knowing something of vegetable physiology and chemistry? —would not a knowledge of scientific botany make a man a better gardener?—is there any county magistrate, or mayor, or alderman of a borough, to whom it would not be useful to know something of the principles on which what are called sanitary measures are to be conducted?—and is there anyone in any situation in life to whom it would not be a benefit to know something of animal physiology, of the functions of his own body, and of the influence which his bodily condition exercises over those moral and intellectual faculties by which he is distinguished from the rest of the animal creation? If it did not teach him how to cure disease, it might be useful for him to know how far disease may cure itself, and what are the limits of Nature in this respect? To man, looking at him as an individual, there is no art so important as that of understanding and managing himself,—an art so simply and well expressed by the two significant words Γνῶθι σεαυτὸν, which were inscribed over the heathen oracle of Delphi. To correct bad habits when once acquired is no very easy task; a strong sense and a strong will, such as only a limited number of persons possess, are necessary for that purpose. But it would go far towards preventing the acquirement of such habits, if young persons, during the period of their education, were made to understand the ill consequences to which they must inevitably lead, and how, eventually, the body must suffer and the mind be stupefied and degraded, not by the reasonable indulgence, but by the abuse of the animal instincts.

In the Introduction to his 'Inquiry into the Human Understanding,' David Hume, having referred to the remarkable progress which had been lately made in a knowledge of astronomy and other physical sciences, has suggested that "the same method of inquiry, which has been applied with so great advantage in these sciences, might also be applied with advantage to those other sciences which have for their object the mental power and economy." I call your attention to this remark, because it brings me to the consideration of another subject, namely, the influence which the pursuit of the physical sciences, conducted as it has been more or less since the days of Galileo and Kepler, has exercised over other studies, and in the advancement of other kinds of knowledge. It needs no argument to prove, for it must be sufficiently plain to everyone, that other sciences as well as the physical have at the present time a very different character from that which they had formerly. It was probably from the operation of various causes (a principal one, however, being the too exclusive and undue importance attached to the Aristotelian logic in the schools), that some centuries had elapsed since the revival of learning before the inductive method (which, by the way, is nothing more than the logic which we all make use of instinctively in the ordinary concerns of life) became generally applied to the investigation of the phenomena and laws of the material universe But a still further time elapsed, even after the publication of Lord Bacon's views on the subject, before other sciences began to partake of this movement, and when they did so, it seems not possible to doubt that it was the result of the impulse which the rapid growth of the physical sciences had communicated to them.

That such was the opinion of David Hume as to the influence thus exercised on one class of inquiries in which he was himself engaged, I have already shown But long before Hume wrote, the same impression had existed on the mind of Locke, as will be sufficiently obvious to anyone on reading the Introductory Chapter of his 'Essay on the Human Understanding' In fact Locke had originally directed his attention to Natural Philosophy and Medicine, and his researches in Moral and Intellectual Philosophy were engrafted on his earlier studies So in the case of Dr Berkeley his treatise on 'Vision' contains the essential part of those doctrines which he afterwards published in his 'Treatise on the Principles of Human Knowledge,' and it is easy to see how, step by step, these gradually arose out of his former studies of Natural Philosophy I make no reference to the modern German school of metaphysicians, and indeed am quite incompetent to do so Neither do I refer to another order of metaphysicians, one of whom informs us how ideas and emotions and volitions are produced by big and little vibrations of the molecules of the nervous system, while another undertakes to explain "the action of material ideas in the mechanical machines of the brain" But with regard to the more eminent of our English writers on these subjects, and what has been called the Scotch school of metaphysicians, including Reid, Adam Smith, Dugald Stewart, and Brown, it may be truly asserted that the advantage which they have had over the dreamy metaphysicians of former times is to be attributed to their having in their mode of inquiry followed the example which had been set them in the study of the physical sciences.

I must not exhaust your patience by going on to explore so wide a field as that on which I have just entered The subject is one to which justice cannot be done without a much more ample discussion than would be convenient on an occasion like the present All that I shall say besides may be comprised in a very few words In composing his 'Essays' on what is now called Political Economy, we may presume that David Hume's mind was influenced by the same considerations as when he composed those other Essays to which I have alluded, and it is not too much to say that these researches of Hume's may be regarded as having, more than anything besides, contributed to lay the foundation of that vast science which has been since developed through the labours of Adam Smith and Horner, and of others who are still alive among us

At the same time, in giving this credit to Hume, we must not overlook what is due to one of our own body, and an original Fellow of the Royal Society Sir William Petty contributed several papers to the 'Philosophical Transactions' In an early part of his life he had been engaged in giving lectures on Anatomy and on Chemistry at Oxford, and his mind having been thus prepared he entered on the consideration of other subjects, such as taxation and trade as affecting the material prosperity of nations, and social statistics His 'Discourse on Political Arithmetic' seems to have been the last result of his labours, it having been first published after his death, by his son, Lord Shelburne In his Preface to this Discourse he thus expresses himself (and I quote the passage because it will serve to show how in these later investigations his mind was influenced by those in which he had been previously engaged) —"The method I take to do this is not very usual · for, instead of using only comparative and superlative words, and intellectual arguments, I have taken the course (as a specimen of the political arithmetic I have long aimed at) to express myself in terms of number, weight, or measure, to use only arguments of sense, and to consider only such causes as have visible foundations in Nature"

It would be easy to adduce from other sciences analogous illustrations of the proposition which I have ventured to advance Compare the natural theology of Derham, Paley, and the Bridgewater Treatises, all founded on the observation of natural phenomena, with the speculations of the ancient philosophers, or with the abstractions and *a priori* arguments of Dr Samuel Clarke Compare the unravelling of early history by Niebuhr and Arnold with anything regarding history that had been done before, or the best practical treatises on politics and government of modern times with the elaborate but fantastic scheme of Plato's republic

If I have made too large a demand on your patience by dwelling on matters which have no special or exclusive relation to our body, you will, I hope, accept it as a sufficient apology that I have done so under the impression that whatever relates to the advancement of knowledge generally cannot be altogether uninteresting to those who are the living representatives of the great men by whom the Royal Society was founded, and who themselves now constitute the most ancient scientific institution in the world

Looking at what more particularly concerns ourselves, I may congratulate you *on the results obtained during the last year* In the volume of the 'Philosophical Transactions' which is now in the course of publication, we find that there is scarcely any department of physical knowledge which is not honourably represented, at the same time that, besides the abstracts of the principal papers, many investigations which have not been deemed to be of sufficient importance, or sufficiently original to have a place in our annual volume, but which nevertheless are of considerable interest, are recorded and published from time to time in the smaller volume bearing the title of 'The Royal Society's Proceedings' By means of this less pretentious publication many facts, many thoughts and suggestions are preserved, which might otherwise have been neglected or lost, but which, being thus preserved, may prove to be of much value hereafter Our weekly meetings have been well attended, and have been rendered more attractive by a practice which is not altogether new, but which has been more generally adopted than heretofore during the last Session, I allude to that of the authors of papers communicated to us giving an oral or *vivâ voce* explanation of their contents,--those explanations being rendered more intelligible by a reference to diagrams, or to the apparatus used for experiments, and even by experiments actually displayed. Such illustrations are useful both to the authors and to others, by causing the subject-matter of the several communications to be better understood, and they are useful in another way, inasmuch as they lead to conversations and discussions, and to the interchange of opinion at the time, from which we may all of us derive something to think of, and reflect on afterwards

Having occupied so much of your time already, I do not feel justified in making a further demand on it by entering into a recapitulation of what has been done in the way of scientific discovery during the last year There is, however, one subject to which I am led to advert because it is of more than usual interest, not only on account of its connexion with scientific investigations, but also on other grounds

After an interval of two years, Captain M'Clintock and those who were associated with him have returned in safety from their voyage of discovery, and their investigations in the Arctic regions The result has been that, although our most earnest wishes have not been realized, it cannot be said that our more reasonable expectations have been disappointed There seemed to be no more than a small probability that any of those who accompanied Sir John Franklin when he quitted his native country in the year 1845 should be still alive in the dreary and inhospitable regions in which, after the loss of their vessels, they had been imprisoned Captain M'Clintock's careful inquiries have fully dissipated whatever faint hopes might have been entertained of its being otherwise, leaving us only the poor consolation of knowing that the sufferings of these gallant spirits are at an end.

As scientific discoverers, Captain M'Clintock and his officers have well fulfilled their mission, as is proved by the magnetic observations which Captain M'Clintock has already communicated to the Royal Society, and of which General Sabine, with his usual perspicuity, gave us some account at one of our evening meetings

In speaking of those engaged in the late expedition, I am unwilling to pass over in silence the name of Mr Young, who, having been the commander of a merchant-ship, took so much interest in the projected enterprise that he not only contributed £500 towards defraying the expenses of it, but volunteered his personal services on the occasion, by acting as master of the vessel Nor ought I to omit to notice the name of Dr. Walker, who, being engaged as surgeon, acted also as naturalist to the expedition, and availed himself of such scanty opportunities as those ice-bound countries afford of extending his researches in natural history Of the results which he has been able to obtain I am not in a condition to give you an account at present, but they will, I doubt not, in due time be communicated to the public

The greatest honour which the Royal Society has to bestow, namely the Copley Medal, has been awarded to Professor Wilhelm Eduard Weber of Gottingen, foreign member of the Royal Society, for his investigations contained in his 'Maasbestimmungen' and other researches in electricity, magnetism, acoustics, &c

The first work in which Professor Weber was engaged was 'The Theory of Undulations,' published in conjunction with his brother Ernest in 1825 This work is still one of standard authority It contains not only a complete account of all that was previously known on the subject of waves in water, but is the repository of many original and important experiments throwing light on this subject The volume contains also many valuable investigations in acoustics Subsequently to this, Professor Weber communicated to Poggendorff's 'Annalen' numerous memoirs containing his further observations in acoustics, among which were his experiments on the longitudinal vibration of rods and strings, on reed organ-pipes, on grave harmonic sounds, and also his method of determining the specific heat of bodies by their sonorous vibrations In this department of physical science he has been a worthy coadjutor of Chladni and Savart.

In association with his brother Edward, then Anatomical Prosector in Leipsic, he in 1835 published the details of an anatomical, physical, and mathematical investigation of the mechanism of the human organs of locomotion, one result of which was the promulgation of a theory of animal progression more nearly in accordance with observed facts than any that had been proposed previously

On his association with M Gauss in the Magnetic Observatory at Göttingen, Professor Weber devoted himself almost exclusively to the subject of magnetism and electricity The annual volumes of the 'Results of the Observations of the Magnetic Union,' published by these eminent philosophers between 1838 and 1843, contain the description of several new instruments, some of which have been the models of those which are now used in all observatories They include also a great variety of important original researches.

It ought not to be omitted that the researches of Gauss and Weber with reference to the transmission of electric signals did more to excite attention to the practicability of an electric telegraph than anything that had been done previously

In 1846 Professor Weber published a memoir on "The Measures of Electro-dynamic Forces" ("Electrodynamische Maasbestimmungen"), a work not less remarkable for the original mathematical than for the experimental researches embodied in it A high authority has pronounced this to be "one of the most important works both with regard to mathematical theory, and the practical application of it, that has been published in this department of science since the researches of Ampère," and the same authority has added, "His transformation of Ampère's law of electric action, so as to exhibit the analyses of the *plus* and *minus* elements in each stream, and his deduction thence of the law of statical from that of dynamical action, seems to me, both as a specimen of mathematical analysis and of physical philosophy, exceedingly beautiful"

More recently Professor Weber has produced two additional memoirs on the same subject, one of which contains a mathematical and experimental investigation of the phenomena of dia-magnetism discovered by Faraday

PROFESSOR MILLER,

As I have not the opportunity of presenting it to him in his own person, I request of you, as Foreign Secretary, to cause the Copley Medal which I now place in your hands to be conveyed to Professor Weber, with a request that he will be pleased to accept it as the indication of the very high estimation in which his scientific labours are held by the Royal Society of London

One of the Royal Medals has been awarded to Arthur Cayley, Esq, F R S, for his Mathematical Papers published in the Philosophical Transactions, and in various English and Foreign Journals

From the first institution of the Royal Society a large proportion of the papers communicated to them have related to Pure Mathematics, and none have contributed more than these to maintain the credit of the Philosophical Transactions Among writers of the present time, no one has been a more earnest or more successful labourer in this department of science than Mr Cayley. His numerous papers on these subjects bear testimony to his unwearied industry, and the undivided opinion as to their value and importance held by those who are best qualified to judge of them, sufficiently establishes Mr Cayley's claim to be regarded as one of the most eminent and profound mathematicians of the age in which we live

Mr. Cayley is among the foremost of those who are successfully developing what may be called the *organic* part of algebra into a new branch of science, as much above ordinary algebra in generality as ordinary algebra is itself above arithmetic The effect is a vast augmentation of our power over the comparison and transformation of algebraical forms, and greatly increased facility of geometrical interpretation

To give any full account of Mr Cayley's labours would be impossible, from mere want of space, and such account, were it given, would be intelligible to none but the highest order of mathematicians, moreover, you are well aware, it could not come from my own knowledge of the subject I have, however, considered it my duty to lay something before you, in the most general terms of description, about these very remarkable papers, obtained from those who are competent to describe them

Mr Cayley's memoirs relate almost exclusively to pure mathematics, and a considerable proportion of them belong to the subject Quantics, defined by him to denote the entire subject of rational and integral functions, and of the equations and loci to which these give rise, in particular the memoirs upon linear transformations and covariants, and many of the memoirs upon geometrical subjects, belong to this head Among the memoirs upon other subjects may be mentioned Mr Cayley's earliest memoir (1841) in the Cambridge Mathematical Journal, "On a Theorem in the Geometry of Position," which contains the solution in a compendious form, by means of a determinant, of Carnot's problem of the relation between the distances of five points in space, the memoir in the same Journal, "On the Properties of a certain Symbolical Expression," which is the first of a series of memoirs upon the attraction of ellipsoids, and the multiple integrals connected therewith, a memoir in Liouville's Journal, which contains the extension of the theory of Laplace's functions to any number of variables, and the memoirs in the same two Journals, on the inverse elliptic integrals or doubly periodic functions. The earliest of the memoirs upon linear transformations was published

(1845 and 1846) in the Cambridge and the Cambridge and Dublin Mathematical Journals, and under a different title in 'Crelle' The antecedent state of the problem was as follows:—The theory of the linear transformations of binary and ternary quadratic functions had been established by Gauss, the same being in fact the foundation of his researches upon quadratic forms, as developed in the 'Recherches Arithmétiques,' and that of the linear transformations of quadratic functions of any number of variables had been considered by Jacobi and others A very important step was made by Mr Boole, who showed that the fundamental property of the determinant (or, as it is now commonly called, discriminant) of a quadratic form applied to the resultant (discriminant) of a form of any degree and number of variables,—the property in question being, in fact, that of remaining unaltered to a factor près, when the coefficients are altered by a linear transformation of the variables, or as it may for shortness be called, the property of invariancy the theorem just referred to, suggested to Mr Cayley the researches which led him to the discovery of a class of functions (including as a particular case the discriminant), all of them possessed of the same characteristic property These functions, called at first hyperdeterminants, are now called invariants, they are included in the more general class of functions called covariants, the difference being that these contain as well the variables as the coefficients of the given form or forms. The theory has an extensive application to geometry, and in particular to the theory of the singularities of curves and surfaces. This theory for plane curves was first established (1834) by Plücker upon geometrical principles, the analytical theory for plane curves is the subject of a memoir by Mr. Cayley in 'Crelle,' and of his recent memoir in the Philosophical Transactions, "On the Double Tangents of a Plane Curve," based upon a Note by Mr. Salmon The corresponding geometrical theory for curves of double curvature and developable surfaces, was first established in Mr. Cayley's memoir on this subject in 'Liouville' and the 'Cambridge and Dublin Mathematical Journal' The theory for surfaces in general, is mainly due to Mr Salmon Among Mr. Cayley's other memoirs upon geometrical subjects, may be mentioned several papers on the Porism of the in-and-circumscribed polygon, and on the corresponding theory *in solido*, a memoir on the twenty-seven right lines upon a surface of the third order, and the memoir in the Philosophical Transactions, "On Curves of the Third Order" The memoirs on Quantics in the 'Philosophical Transactions' (forming a series not as yet completed) comprise a reproduction of the theory of covariants, and exhibit the author's views on the general subject Mr Cayley has written also a Report on the recent progress of theoretical Dynamics, published in the 'Reports of the British Association' for 1857

MR CAYLEY,

In the name of the Royal Society of London, I request your acceptance of this Royal Medal, in testimony of the strong sense which they entertain of the value of your labours, and of the satisfaction which it affords them that so eminent a mathematician as yourself should be included in the list of their Fellows

The other Royal Medal has been awarded to Mr. George Bentham, F L S, for his important contributions to the advancement of Systematic and Descriptive Botany

The remarkable accuracy which distinguishes all Mr Bentham's scientific researches, the logical precision that characterizes his writings, and the sound generalizations which his systematic works exhibit may be in a great measure traced to the influence of his uncle, the late celebrated legal theorist Jeremy Bentham, who directed much of his early studies, and under whose auspices he published one of his earliest works, 'Outlines of a New System of Logic.' His mind was further imbued in youth with a love of Natural History, and especially Botany, and this taste was cultivated and nourished by a study of the works of the elder DeCandolle.

Fortunately for the cause of Botany in England, Mr. Bentham has devoted himself almost exclusively to that science, and to his excellent powers of observation, close reasoning, concise writing, and indefatigable perseverance our country owes the distinction of ranking amongst its Naturalists one so preeminent for his valuable labours in Systematic Botany.

Amongst Mr. Bentham's numerous writings, those hold the first rank which are devoted to the three great natural orders, Leguminosæ, Labiatæ, and Scrophulariaceæ. These orders demanded a vast amount of analytic study; for they are amongst the largest and most widely distributed of the vegetable kingdom, and had been thrown into great confusion by earlier writers They have been the subject of many treatises by Mr Bentham, and especially of two extensive works, the contents of which have lately been embodied in the "Systema Vegetabilium" of the DeCandolles On their first appearance these works secured for their author a European reputation, and will always rank high as models of skilful classification

It would occupy too much time to specify the very numerous monographs and papers which Mr Bentham has communicated to various scientific societies and periodicals in this country and on the Continent, and especially to the Linnean Transactions and Journal That "On the Principles of Generic Nomenclature" may be noted as an example of his power of treating an apparently simple, but really abstract and difficult subject in a manner at once philosophical and practical Mr Bentham's most recent work, that on British Plants, is the first, on the indigenous Flora of our Islands, in which every species has been carefully analysed and described from specimens procured from all parts of the globe, it is distinguished for its scientific accuracy, advanced general views, and extreme simplicity, —a combination of qualities which can result only from an extensive series of exact observations, judiciously arranged and logically expressed

MR. BENTHAM,

The early volumes of the 'Philosophical Transactions' contain numerous papers relating to Botany and the other sciences which are usually comprehended under the general designation of Natural History. As these sciences, but especially Botany, became more and more extended, it was thought desirable that another Institution should be called into existence, which might share with the Royal Society the privilege of promoting the cultivation of them, and of communicating to the world from time to time the progress which has been made in this department of knowledge and such was the origin of the Linnean Society in the year 1788 The Royal Society, however, does not on that account feel the less interest in this class of scientific investigations. It is accordingly with great satisfaction that the Council have awarded to you one of the Royal Medals, and that, in the name of the Society, I now place it in your hands, in testimony of their high appreciation of your researches, and of the respect which they have for you as a fellow-labourer in the field of science

On the motion of Mr. Faraday, seconded by Sir Henry Holland, the best thanks of the Society were voted to the President for his excellent address, and he was requested to permit the same to be printed.

[For the Obituary Notices of Deceased Fellows the reader is referred to the end of the Volume]

The Statutes relating to the election of Council and Officers having been read, and Mr Christie and Dr Mayo having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected

The following Fellows were reported duly elected Officers and Council for the ensuing year —

President —Sir Benjamin Collins Brodie, Bart , D C L.

Treasurer —Major General Edward Sabine, R A., D.C.L.

Secretaries —William Sharpey, M D
George Gabriel Stokes, Esq , M A., D C.L.

Foreign Secretary.—William Hallows Miller, Esq , M A

Other Members of the Council —C Cardale Babington, Esq , M A ; Rear-Admiral Sir George Back, D C L , Rev. John Barlow, M A., Thomas Bell, Esq., Arthur Cayley, Esq ; William Farr, M D., D.C.L , Sir H. Holland, Bart , M D , D.C L ; Thomas Henry Huxley, Esq., Sir Roderick I. Murchison, M A , D.C.L. ; Thomas Webster, Esq , M A. ; Rev. William Whewell, D D. ; Alex William Williamson, Ph D. ; Rev. Robert Willis, M A., Sir William Page Wood, D C L , The Lord Wrottesley, M.A , Colonel Philip Yorke.

Receipts and Payments of the Royal Society between November 30, 1858, and December 1, 1859

	£	s.	d
Subscriptions and Compositions	1558	4	0
Rents	192	4	9
Dividends on Stock, including Trust Funds	1141	3	11
Sale of Transactions, Proceedings, &c.	372	8	1
Cost of Printing Paper "Astronomical Expedition to the Peak of Teneriffe" (repaid by Admiralty)	135	1	9
Stevenson Bequest	3466	3	2
Chemical Society	99	7	9
Geographical Society	33	10	0
Linnean Society	17	17	0

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16s. per annum

Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £110 0s. 0d per annum

Fee farm rent in Sussex, £19 4s. per annum

One-fifth of the clear rent of an estate at Lambeth Hill from the College of Physicians, £3 per annum

£14,000 Reduced 3 per cent. Annuities.

£26,476 19s. 1d Consolidated Bank Annuities.

£513 9s. 8d New 2½ per cent. Stock.

EDWARD SABINE,
Treasurer

£7016 0 5

	£	s.	d
Balance due to Bankers	155	8	0
Ditto to Treasurer	27	18	8
Rev H. Stebbing, Fairchild Lecture	2	18	9
Dr Frankland, Bakerian Lecture	3	18	4
Salaries, Wages, and Pension	766	19	0
Fire Insurance	42	1	6
Printing Transactions	309	4	0
Ditto Proceedings and Miscellaneous	280	16	3
Engraving	651	17	10
Paper for Proceedings	88	16	0
Binding Transactions	64	7	2
Books Purchased and Binding	295	14	4
Furniture	206	9	6
Stationery	16	2	9
Shipping Expenses	3	2	0
Fire and Lighting	123	3	6
House Expenses	77	9	5
Taxes	11	1	8
Donation Fund	25	0	0
Wintringham Fund	35	2	0
Copley Medal Fund	5	1	6
Rumford Medal Fund	140	10	5
Catalogue of Scientific Periodicals	194	15	8
Postage, Miscellaneous and Petty Charges	85	7	4
Testimonial to J Balmat	26	5	0
Cleaning Pictures	251	18	0
Law Expenses	26	11	10
Purchase of £2907 2s 1d 3 per cent. Consols	2700	0	0
Balance at Bankers	420	0	0
	£7016	0	5

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Honorary	Foreign	Having compounded	Paying £2 12s annually	Paying £4 annually	Total
December 1, 1858	9	50	365	7	275	706
Since elected			+ 4		+13	+17
Since deceased	−2	−3	−16		−11	−32
November 30, 1859	7	47	353	7	277	691

December 8, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The President announced that, under the provisions of the Charter, he had appointed the following Members of the Council to be Vice-Presidents :—

Thomas Bell, Esq.
Sir Roderick I. Murchison
Major-General Sabine.
The Rev. William Whewell, D.D.
Sir William Page Wood.
The Lord Wrottesley.

The following communications were read —

I "On the Analytical Theory of the Attraction of Solids bounded by surfaces of a Class including the Ellipsoid" By W F Donkin, Esq, M.A, F.R.S, F.R.A S, Savilian Professor of Astronomy in the University of Oxford. Received September 2, 1859

(Abstract)

The surface of which the equation is

$$f(x, y, z, h, k)=0, \quad . \qquad . \qquad (1)$$

is called for convenience "the surface (h, k)" The space, or solid, included between the surfaces (h_1, k), (h_2, k), is called "the shell $\left({h_2 \atop h_1}, k\right)$," and that included between the surfaces (h, k_1), (h, k_2) is called "the shell $\left(h, {k_2 \atop k_1}\right)$" [This notation is borrowed, with a slight alteration, from Mr Cayley] It is assumed that the equation (1) represents closed surfaces for all values of the parameters h, k, within certain limits, and that (within these limits) the surface (h, k) is not cut by either of the surfaces $(h+dh, k)$, $(h, k+dk)$ It is also supposed that there exists a value h_∞ of h, for which the surface (h_∞, k) extends to infinity in every direction Lastly, it is supposed that if k be considered a function of x, y, z, h, by virtue of (1), the two following partial differential equations are satisfied

$$\frac{d^2k}{dx^2}+\frac{d^2k}{dy^2}+\frac{d^2k}{dz^2}=\phi(h),$$

$$\left(\frac{dk}{dx}\right)^2+\left(\frac{dk}{dy}\right)^2+\left(\frac{dk}{dz}\right)^2+n\,\frac{dk}{dh}=0\,;$$

in which $\phi(h)$ is any function of h (not involving k), and n is any constant independent of h and k. The following propositions are then demonstrated :—

The potential, on a given external point, of a homogeneous solid bounded by the surface (h, k), varies as the mass of the solid, if h vary while k remains constant

The potentials, on a given external point, of the homogeneous shells $\left(h_1, {k_2 \atop k_1}\right)$, $\left(h_2, {k_2 \atop k_1}\right)$ are proportional to the masses of the shells, The homogeneous shell $\left(h, \frac{k_2}{k_1}\right)$ exercises no attraction on an interior mass.

The external equipotential surfaces of the homogeneous infinitesimal shell $\left(h_2, \frac{k}{k}+dk\right)$, are the surfaces (h, k), in which h is arbitrary and k invariable[*]

The potential of the homogeneous infinitesimal shell $\left(h_2, \frac{k}{k}+dk\right)$ upon an *exterior* point, is

$$\frac{4\pi}{n}\,dk\,\psi(h_2)\int_{h}^{h_\infty}\frac{dh}{\psi(h)},$$

and upon an *interior* point, is

$$\frac{4\pi}{n}\,dk\,\psi(h_2)\int_{h_2}^{h_\infty}\frac{dh}{\psi(h)}$$

(In these expressions $\psi(h)$ is $\epsilon^{\frac{1}{n}\int\phi(h)dh}$, and h at the lower limit in the first, is the parameter of the surface (h, k) which passes through the attracted point The density of the shell is supposed to be unity)

The potential of the finite homogeneous shell $\left(h_2, \frac{k''}{k'}\right)$ (density $=1$) upon an exterior point (ξ, η, ζ), is

$$\frac{4\pi}{n}\psi(h_2)\left\{k''\int_{h''}^{h''_\infty}\frac{dh}{\psi(h)}-k'\int_{h'}^{h_\infty}\frac{dh}{\psi(h)}+\int_{h'}^{h''}\frac{k\,dh}{\psi(h)}\right\}.$$

in this expression it has been assumed (for simplicity) that h_∞ is independent of k. Also h'', h' are the values of h corresponding to k'', k', when h and k vary subject to the relation $f(\xi, \eta, \zeta, h, k)=0$; and k, in the last integral, is the function of h, ξ, η, ζ determined by this relation

The differential equations (2) are satisfied in the case of the ellipsoid. For if we put its equation in the form

$$\frac{x^2}{a^2+h}+\frac{y^2}{b^2+h}+\frac{z^2}{c^2+h}=k,$$

it is evident on inspection that

$$\frac{d^2k}{dx^2}+\frac{d^2k}{dy^2}+\frac{d^2k}{dz^2}=2\left(\frac{1}{a^2+h}+\frac{1}{b^2+h}+\frac{1}{c^2+h}\right),$$

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[*] It is known that the last two propositions imply the first two (see Mr. Cayley's "Note on the Theory of Attraction," Quarterly Journal of Mathematics, vol. ii. p. 338); though this is not the order of proof in the present paper.

and

$$\left(\frac{dk}{dx}\right)^2+\left(\frac{dk}{dy}\right)^2+\left(\frac{dk}{dz}\right)^2+4\frac{dk}{dh}=0$$

In this case we find $\psi(h)=\left((a^2+h)(b^2+k)(c^2+h)\right)^{\frac{1}{2}}$, and the above general expressions lead to the known results.

II Supplement to a Paper, read January 27, 1859, "On the Thermodynamic Theory of Steam-engines with dry Saturated Steam, and its application to practice." By W. J MACQUORN RANKINE, C.E, F R S. &c *

(Abstract)

This supplement gives the dimensions, tonnage, indicated horsepower, speed, and consumption of fuel, of the steam-ships whose engines were the subjects of the experiments referred to in the original paper. Results are arrived at respecting the available heat of combustion of the coal employed, and the efficiency of the furnaces and boilers, of which the following is a summary —

No of experiment	Kind of boiler	Total heat of combustion of 1 lb of coal in ft lbs., estimated from chemical composition	Available heat of combustion of 1 lb of coal in ft -lbs computed from efficiency of steam and weight of coal burned per I H P.	*Available heat*, total heat, = efficiency of furnace and boiler
I	Improved Marine Boilers of ordinary proportions.	10,000,000	5,420,000	0·542
III		10,000,000	5,300,000	0 53
II.	Boiler chiefly composed of small vertical water-tubes, with very great heating surface.	11,560,000	10,110,000	0 88

Available Heat of Combustion of 1 lb of coal

$$=\frac{1{,}980{,}000 \text{ ft.-lbs.}}{\text{Efficiency of steam} \times \text{lb. coal per I H. P per hour}}.$$

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* Phil. Trans. 1859, p 177; and Proceedings of the Royal Society, vol. ix p. 626.

III. Supplement to a Paper, read February 17, 1859, "On the Influence of White Light, of the different Coloured Rays and of Darkness, on the Development, Growth, and Nutrition of Animals*." By HORACE DOBELL, M D &c Communicated by JAMES PAGET, Esq Received September 23, 1859.

The apparatus used in the following experiments, was described in my Paper, but in the present instance, only two of the cells were employed, viz that exposed to ordinary white light, and that from which all light is excluded In order more effectually to prevent the possible admission of light, the following precautions were adopted with the dark cell —1 The perforated zinc floor was covered with thick brown paper 2. The under surface of the lid was lined with black cloth, to secure accurate adjustment when shut 3. The opaque black glass was covered with an additional coat of black oil-paint 4 The lid was never opened in any light except that of a candle or of gas.

March 20th, 1859 —A number of ova of the Silkworm (*Bombyx mori*), all of the same age, were placed in each of the two cells. No change was observed until *May* 18*th* (sixty days after the commencement of the experiments), when one larva emerged from the ovum in each cell, and during twelve days, larvæ continued to emerge in the light and in the dark at the same rate.

June 9th.—Sixteen larvæ, as nearly as possible of the same size, were selected in each cell, and the rest removed. The experiments then proceeded with these thirty-two individuals, and no death occurred from first to last

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* Proceedings of the Royal Society, vol ix p. 644

The following Table shows the day on which each larva began to spin, the day on which the perfect insect escaped from the pupa, and hence the number of days occupied by the metamorphosis

Light			Darkness		
Day of beginning to spin	Day of escape of the Moth	Number of days occupied by metamorphosis	Day of beginning to spin	Day of escape of the Moth	Number of days occupied by metamorphosis
July 1	July 18	18 days inclusive	June 30	July 18	19 days inclusive
,, 2	,, 19	18 ,, ,,	,, 30	,, 18	19 ,, ,,
,, 2	,, 19	18 ,, ,,	,, 30	,, 18	19 ,, ,,
,, 2	,, 18	17 ,, ,,	,, 30	,, 18	19 ,, ,,
,, 2	,, 18	17 ,, ,,	,, 30	,, 21	22 ,, ,,
,, 2	,, 19	18 ,, ,,	July 1	,, 18	18 ,, ,,
,, 2	,, 19	18 ,, ,,	,, 1	,, 18	18 ,, ,,
,, 3	,, 19	17 ,, ,,	,, 2	,, 18	17 ,, ,,
,, 3	,, 21	19 ,, ,,	,, 2	,, 19	18 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 20	19 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 19	18 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 20	19 ,, ,,
,, 4	,, 21	18 ,, ,,	,, 2	,, 21	20 ,, ,,
,, 4	,, 21	18 ,, ,,	,, 3	,, 21	19 ,, ,,
,, 5	,, 21	17 ,, ,,	,, 3	,, 20	18 ,, ,,
,, 6	,, 24	19 ,, ,,	,, 4	,, 21	18 ,, ,,

From this it is seen that the mean period occupied by the metamorphosis in the *darkened cell* was eighteen days fifteen hours, and in the *light cell* seventeen days sixteen hours.

The longest and shortest periods in the *darkened cell* twenty-two days and seventeen days, in the *light cell* nineteen days and seventeen days.

June 9th.—On selection of sixteen of the largest larvæ from the inhabitants of each cell, it was noted that, when sixteen were selected from the *darkened cell* and several of *similar size* removed, only four could be found as large in the *white cell*, the remaining twelve selected were therefore of a rather smaller size This difference in the two cells became less obvious afterwards, but, throughout the experiments, there was a slight difference of size in favour of the darkened cell.

With these exceptions, no difference could be detected between the results obtained in the cell from which light was completely excluded and in that exposed to its full influence

The larvæ, the silk produced, and the moths from the two cells, when placed side by side, could not be distinguished from one another

The ova were of the same colour when first deposited, and underwent the same changes of appearance, at the same time, in the dark and in the light

So far, therefore, as the direct agency of light is concerned in the development, growth, nutrition, and coloration of animals, the results of these experiments closely correspond with those already recorded in my Paper.

IV "On the Effects produced in Human Blood-corpuscles by Sherry Wine, &c" By William Addison, Esq, F R S, Fellow of the Royal College of Physicians, London

Received September 10, 1859

(Abstract)

The author has found that when a small drop of fresh blood is placed beside a similar drop of sherry wine on a slip of glass, and viewed with the microscope, after being covered as usual with a thin piece of glass, certain changes are seen to take place in the blood as it mingles with the wine, which are thus described.—

"In those parts where the wine is mingling with the blood—at the outer edges of the mass—various altered corpuscles will be seen They float in the fluid, separated from each other, having now no longer any disposition to adhere together in rolls Their outlines are altered, and sundry markings appear in their interior After a short time—perhaps ten minutes, sometimes sooner—numerous corpuscles will be observed throwing out matter from their interior; two, five, or ten molecular spots fringing their circumference. Some of these molecules grow larger and seem coloured, others of them elongate into tails or filaments, which frequently attain to an extraordinary length, and wave about in a very remarkable manner. They all terminate, at the extremity farthest from the corpuscle, in a round globular enlargement A single corpuscle may very frequently be seen with five or six of these tails.

"During the observation of these phenomena, numerous molecular particles are seen continually passing from the corpuscles; they

float about in, and disturb the transparency of the fluid moreover, they have an extremely vivid movement.

"At the expiration of half an hour, many of the tails or filaments are seen assuming the form of a necklace of beads, and those also, separating from the corpuscles, float about with a singular independent movement in the fluid"

When blood is similarly mingled with a fluid consisting of two parts of sherry wine and one part of a solution made with a grain and a half of common salt and a grain of bicarbonate of soda to half a fluid ounce of water, the transparency of the fluid part of the blood is not altered, "but the corpuscles are changed in appearance, and the tails or filaments which are now seen issuing from them are generally thicker and much more conspicuous than when the wine alone is used The molecules which separate from the corpuscles are larger, and the tails which break away from the corpuscles upon any slight motion of the fluid, present various dumb-bell, necklace-like, serpentine, globular, and other shapes"

Under particular but accidentally produced conditions of the mingled fluids, the author has "repeatedly seen the tails suddenly retract, not into the interior of the corpuscle, but into a globular ball at its side Sometimes the tail shortens to only half its length, becoming in a corresponding degree thicker" The tail in thus shortening may become bulged in the middle of its length, exhibiting at that part a globular or discoid enlargement This globule or disc may then burst, and in such case the blood-corpuscle finally exhibits only a small round particle remaining at the point of its circumference from which the tail had proceeded

After describing in detail various other appearances which he noted in his experiments, and which, as well as those above mentioned, are delineated in several drawings which illustrate the paper, the author thus states his views as to the nature of the phenomena observed :—

"In these experiments, a mixture of a saline solution and sherry wine, or the wine alone, is added to a drop of fresh blood. The addition must change the properties of the fluid in which the corpuscles naturally swim. The change in the fluid produces changes in the corpuscles, shown by their altered appearance, their indisposition any longer to adhere in rolls, and the various markings seen within them

Some time after these changes the corpuscles discharge molecular particles, and emit tails or filaments, which, separating from the corpuscles, materially alter the transparency and aspect of the fluid

"The first alteration of the fluid element of the blood—that, namely, produced by the addition of the extraneous fluid—causes no visible troubling or change in it, but the second alteration, which consists in the appearance of a great number of molecular particles, is visibly produced by the agency of the corpuscles The molecular particles are seen coming out of the corpuscles, separating from them, and disturbing the transparency of the fluid"

In corroboration of his opinion that the filaments and molecules are emitted from the blood-corpuscles, and not produced by a precipitation or solidification of coagulable matter in the plasma, the author especially draws attention to the fact that, so far as he observed, the molecules appear only in those parts of the fluid where the corpuscles have been altered and are fringed with similar molecules, or emitting tails, whilst in other parts, where such changes are not occurring in the corpuscles, the fluid is perfectly clear, and free from molecules. Moreover, the emission of tails and molecules is not the result of a breaking up of the corpuscles, for many of the latter may be seen emitting long tails without any alteration of their natural form, and although no doubt the corpuscles are finally broken up, this process does not take place sooner in those with tails than in those which have none.

In connexion with his present observations, the author relates two cases of febrile and inflammatory disease (already reported by him in the 'London Medical Gazette' some years since), in which molecular matter existed abundantly in the liquid part of the blood The molecules in these cases, as in his present experiments, he believes to have proceeded from the blood-corpuscles, and likewise through the operation of some abnormal influence, which, however, must have acted upon the corpuscles during their circulation in the living body.

The author next refers to certain inferences from the foregoing observations calculated to elucidate questions in pathology and practical medicine, which, however, he has already made known in his Gulstonian Lectures.

To produce the effects described, brown sherry of the best quality was employed. Inferior sherry wines alter the outline of the corpuscles, but do not cause the production either of tails or molecules. With other sherry wines of a better kind, the author finds it preferable to mix them with a fourth or a fifth part of the saline solution instead of one-third, he has tried port-wines and various mixtures of brandy and water, with and without sugar, but almost always without the effect here described

V "Researches on the Phosphorus-Bases"—No. VII. Triphosphonium-Compounds. By A W. Hofmann, LL.D., F R.S. &c Received October 18, 1859

In several previous communications I have submitted to the Royal Society the results which I have obtained in examining the deportment of triethylphosphine with dibromide of ethylene, as the prototype of diatomic bromides I have shown that the final product of this reaction is a diatomic salt corresponding to two molecules of chloride of ammonium

The further prosecution of the study of triethylphosphine in this direction has led me to investigate the derivatives generated by the phosphorus-base, when submitted to the action of triatomic chlorides, bromides, and iodides

The most accessible terms of this group being chloroform, bromoform, and iodoform, the changes of triethylphosphine under the influence of these agents have more especially claimed my attention.

Action of Iodoform on Triethylphosphine

Both substances unite with energy at the common temperature. In order to avoid the inflammation of the phosphorus-base, small quantities of the materials should be mixed at a time The products of the reaction vary with the relative proportions of the two substances.

By adding gradually crystals of iodoform to a moderate bulk of triethylphosphine until a new addition produces no longer an elevation of temperature, a viscous mass of a clear yellow colour is obtained, which, when treated with alcohol, changes to a white powder of crystalline aspect, these crystals are easily soluble in water, difficultly soluble in alcohol, and insoluble in ether. Two or three crystallizations from boiling alcohol render them perfectly pure The analysis of this body has led me to the formula

$$C_{38}\,H_{46}\,P_3\,I_3,$$

which represents a compound of one molecule of iodoform, and three molecules of triethylphosphine,

$$\underset{\text{Triethyl-phosphine}}{3\,C_{12}\,H_{15}\,P} + \underset{\text{Iodoform}}{C_2\,H\,I_3} = \underset{\text{New Compound}}{C_{38}\,H_{48}\,P_3\,I_3}$$

Iodoform thus fixes three molecules of triethylphosphine, giving rise to the formation of the tri-iodide of a triatomic metal, of a triphosphonium corresponding to three molecules of chloride of ammonium

$$C_{38}\,H_{48}\,P_3\,I_3 = \left[\left.\begin{matrix}(C_2\,H)''' \\ (C_4\,H_5)_3 \\ (C_4\,H_5)_3 \\ (C_4\,H_5)_3\end{matrix}\right\}P\,3\right]'''I\,3$$

The aqueous solution of the iodide yields with iodide of zinc a white crystalline precipitate which is difficultly soluble in water, and appears to be slightly decomposed by recrystallization It consists of one molecule of the triatomic iodide and three molecules of iodide of zinc,

$$C_{38}\,H_{46}\,P_3\,I_3,\ 3\,Zn\,I.$$

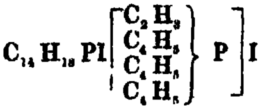
By treating the tri-iodide with the various salts of silver, a series of triatomic compounds is easily obtained, which contain the different acids

The trichloride furnishes with dichloride of platinum a pale yellow precipitate, which is insoluble in water, but dissolves in boiling concentrated hydrochloric acid. From this solution it is deposited on cooling in brilliant rectangular plates, which contain

$$C_{38}\,H_{46}\,P_3\,Cl_3,\ 3\,Pt\,Cl_2.$$

I have vainly tried to produce a trioxide which would correspond to the tri-iodide.

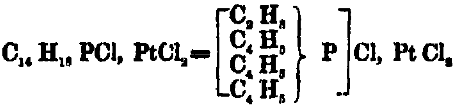
The tri-iodide is promptly attacked by oxide of silver, with formation of iodide of silver, and of an exceedingly caustic fixed base, which remains in solution This base no longer belongs to the same series. By treating its solution with hydriodic acid, or with hydrochloric acid and dichloride of platinum, it is at once perceived that the action of the oxide of silver has profoundly changed the original system of molecules Hydriodic acid no longer produces the salt difficultly soluble in alcohol, by evaporating the solution a crystalline residue is obtained, which easily separates into a viscous, extremely soluble substance, and splendid crystals of an iodide, very soluble in water and alcohol, but insoluble in ether. The analysis of this iodide has proved it to contain



$$C_{14}H_{18}PI \left[\left.\begin{matrix} C_2H_3 \\ C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} P \right] I$$

This formula represents the iodide of methyl-triethylphosphonium, which was formerly obtained by M Cahours and myself, by acting with iodide of methyl upon triethylphosphine.

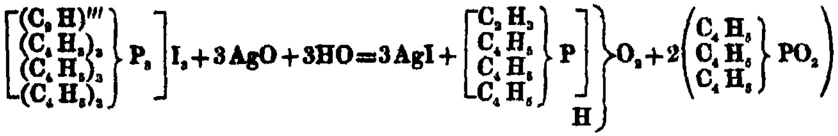
The alkaline liquid, obtained by the action of oxide of silver upon the tri-iodide, when saturated with hydrochloric acid, yields no longer the platinum salt, difficultly soluble in water but soluble in hydrochloric acid. In a dilute solution no precipitate whatever takes place, and only after considerable evaporation well-defined deep orange-yellow octahedrons are deposited, which contain



$$C_{14}H_{18}PCl, PtCl_2 = \left[\left.\begin{matrix} C_2H_3 \\ C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} P \right] Cl, PtCl_2$$

From these results it is obvious that the triphosphonium-salt, when submitted to the action of oxide of silver, passes over into a monophosphonium-compound The latter is not the sole product of the reaction, I have already alluded to the viscous deliquescent substance which accompanies the iodide of methyl-triethylphosphonium This is an iodide, which, in the solution produced by the action of oxide of silver upon the original tri-iodide, exists in the form of oxide The latter substance is easily recognized by evaporating the solution of oxide of methyl-triethylphosphonium, and adding a concentrated solution of potassa, when the oily globules characteristic of the dioxide of triethylphosphonium separate, which disappear immediately on addition of water

The metamorphosis of the tri-iodide, under the influence of oxide of silver, is represented by the following equation:—



$$\left[\left.\begin{matrix}(C_2H)'''\\(C_4H_5)_3\\(C_4H_5)_3\\(C_4H_5)_3\end{matrix}\right\}P_3\right]I_3+3AgO+3HO=3AgI+\left.\begin{matrix}\left[\left.\begin{matrix}C_2H_3\\C_4H_5\\C_4H_5\\C_4H_5\end{matrix}\right\}P\right]\\H\end{matrix}\right\}O_2+2\left(\left.\begin{matrix}C_4H_5\\C_4H_5\\C_4H_5\end{matrix}\right\}PO_2\right)$$

The tri-iodide which forms the subject of this Note is not the only product of the reaction between iodoform and triethylphosphine There are other compounds formed, especially when the iodoform is employed in great excess. The nature of these bodies, which may be divined from the examination of the corresponding compounds in the diatomic series, is not yet fixed by experiment

I have satisfied myself that chloroform and bromoform act like iodoform upon triethylphosphine

The phosphorus-base acts, even at the common temperature, upon tribromide of allyl The mixture of the two bodies solidifies into a crystalline mass, in the examination of which I am engaged

The reactions which I have pointed out in this Note have induced me to extend my experiments to tetratomic bodies The chloride of carbon, C_2Cl_4, obtained by the final substitution of chlorine for the hydrogen in marsh-gas, appeared to promise accessible results On submitting this body, remarkable for its great indifference under ordinary circumstances, to the influence of triethylphosphine, I have observed with astonishment a most powerful reaction Every drop of triethylphosphine which is poured into the chloride of carbon, hisses like water falling upon red-hot iron. On cooling, the mixture solidifies into a mass of white crystals, which will be the subject of a special communication

December 15, 1859.

Sir BENJAMIN C BRODIE, Bart., President, in the Chair.

Samuel Husbands Beckles, Esq., was admitted into the Society.

In accordance with Notice given at the last Meeting, the Right Honourable Edward Lord Stanley, Member of Her Majesty's Privy Council, was proposed for election and immediate ballot; and the ballot having been taken, his Lordship was declared duly elected.

The following communications were read:—

I "Note respecting the Circulation of Gasteropodous Mollusca and the supposed Aquiferous Apparatus of the Lamellibranchiata." By M H LACAZE DUTHIERS. Communicated by Professor HUXLEY. Received October 19, 1859

A memoir upon the aquiferous system and the oviducts of Lamellibranchiate Mollusks by Messrs Rolleston and Robertson, was read before the Royal Society at the Meeting on the 3rd of February, 1859 The abstract of this memoir, contained in the 'Annals and Magazine of Natural History,' reached me in the month of July, and I was not a little surprised to find that a structure which I had so elaborately studied in the course of my various journeys to the sea-shore, and which I had carefully described in a number of species, was something quite different from what I had imagined it to be. Without entering into minute anatomical details, which would not tend to elucidate the question, I find that Messrs Rolleston and Robertson consider that the organs, the ducts, and the orifices supposed to be the ovaries or their excretory ducts, are, in fact, nothing but an aquiferous apparatus, and that the openings placed on each side of the foot are the excretory orifices of this system. They discover elsewhere the ducts whose office is to convey away the products of the genital glands The enunciation of an opinion so opposed to what I, in common with many other authors, had maintained, seemed to require a recurrence to direct observation But on repeating my examination of *Cardium edule, Tellina solidula, Mactra stultorum,* and *Donax anatina,* I have precisely verified my previous conclusions.

On throwing injections into the genital orifices, the sexual glands have become turgid; and on examining fragments of such injected genital glands microscopically, the injected substance was seen mixed with the ova or spermatozoa. These facts may be observed with especial ease in *Cardium edule.*

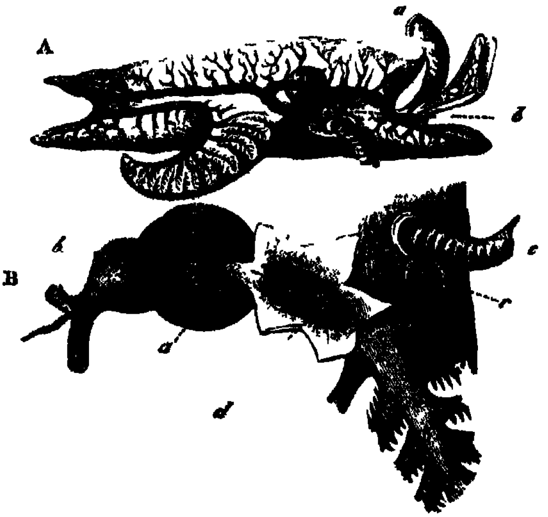
In addition to this, I have seen ova actually laid by living females of *Modiolæ* and *Mytili,* one of the valves of whose shell was removed, on irritation of the genital orifice, and in others the ova or the spermatic fluid may be made to pass out of their orifices, at the breeding season, by pressing gently upon the foot.

In *Spondylus gæderopus* the genital orifice is situated in the sac of Bojanus, and I had great difficulty in finding it when investigating this subject It was, in fact, only by chance that I opened the sac of Bojanus and observed a little rose-coloured cylinder issuing from an orifice in its interior This cylinder, like a thread of vermicelli in aspect, was composed of reddish ova mixed with mucus, and agglomerated I might multiply examples, but it seems to be useless to do so, for I should simply reproduce the facts which I have brought forward in the memoir which I published in the 'Annales des Sciences Naturelles,' on my return from a long stay in the Balearic isles

In this memoir, besides, I have not merely drawn attention to the circumstance, that after oviposition the aspect of the gland changes completely, which might lead an observer to mistake the apparatus of reproduction for something quite different, but I have given figures of this condition of *Pecten varius*, &c In fine, I believe that the structure of the male and female organs of the Lamellibranchiate Mollusca is such as it was described to be before the observations of Messrs Rolleston and Robertson

But does the system of aquiferous vessels, whose occurrence in the Mollusca has been sometimes admitted, sometimes denied, really exist in the Acephala? In the abstract to which I refer, those citations, which doubtless existed in the memoir, have not, and probably could not have appeared It is well known that the belief in this supposed aquiferous system has been gradually becoming weaker The necessity of explaining the extreme dilatation and contraction of the bodies of molluscous animals led anatomists to seek for and describe such a system, but at present the explanation of these facts is found in the direct mixture of water with the blood, or the ejection of the latter liquid. MM Leuckart and Gegenbaur have made observations tending to prove the occurrence of this process in the Pteropods, M Langer of Vienna has published a special memoir on the circulation of *Anodon*, and the great point he makes out is the passage of water into the blood by the intermediation of the organ of Bojanus. I believe that I have demonstrated in *Dentalium* the orifices by which the direct communication of the circulatory apparatus with the exterior of the body takes place, and lastly, I have found a Gasteropod, and one which assuredly occupies a very high place in that group, which presents the same arrangement.

I hope to be able before long to bring out a complete Monograph of the anatomy of *Pleurobranchus aurantiacus*, in which the existence of an external orifice of the circulatory apparatus shall be put beyond doubt As the figure which accompanies this Note shows, a very small orifice (*b*, fig A, and *c*, fig B) with a raised rim is visible above the external genital organs and in front of the principal branchial vein This orifice, hidden by the contractions of the body, is very conspicuous in the dead animal On injecting milk or any other liquid by it, the fluid is seen always to enter the heart, and on slitting up the branchial vein, there is seen within it an aperture (*d*, fig B) leading into a little canal which is connected with the external aperture, and is the channel whereby the fluid injected enters the heart. I have varied the method of injection in every possible way, and always with the same result I cannot conceive that there is any rupture of the parts or any extravasation of the injection, so that I believe (as may be verified in spirit specimens) that in the *Pleurobranchus* the circulatory apparatus communicates directly with the exterior



The demonstration of a direct communication between the exterior and the circulatory apparatus, renders the assumed existence of an aquiferous system *a priori* less necessary, in order to explain the great changes of volume of the body of Mollusks But I believe that, in addition, microscopic examination will show the direct continuity of the genital glands with the lateral orifices placed at the base of the foot in the Lamellibranchiata

This communication of the vascular apparatus with the external water, has a very important bearing on the history of the nutritive processes The physiological conceptions derived from the study of the higher animals are singularly affected by finding creatures which can at will throw out a portion of their blood, or, on the contrary, dilute with water that which is, *par excellence*, the nutritious element.

This would be sufficient to prove, were it necessary to do so, how wide is the difference between the vital processes of the lower and of the higher animals

EXPLANATION OF THE FIGURES

A *Pleurobranchus aurantiacus*, seen in a side view
  a Heart
  b External orifice of the sanguiferous system, placed before the branchiæ and above the genital organ
B Enlarged view of the heart, branchial vein, &c
  a Auricle
  b Ventricle
  c External opening through which fluid may be injected into the heart
  d Branchial vein laid open at this part to show the internal opening of the canal which leads from the external orifice *c*
  e Penis
  f Part of the branchial vein, unopened

II. "On the Repair of Tendons after their subcutaneous division" By Bernard E. Broadhurst, Esq., F.R C S Communicated by T Blizard Curling, Esq Received November 4, 1859

(Abstract)

The results of the experiments which are recorded by the author are divided into three classes, which tend to show—

1st That a tendon, having been divided, may reunite without leaving permanently a cicatrix

2ndly That the uniting new material may be drawn out to any required length, and in such case may, under gradual and carefully regulated extension, even acquire the thickness of the tendon itself, but that if the divided ends are widely separated after the section, and so remain, reunion will not take place

3rdly. That the addition of new tendon does not impair the strength of the muscle, unless the length be more than sufficient, in which case it occasionally weakens the muscle

The process of reunion is explained, and the appearances presented by the tendon in the various stages of reunion are detailed and illustrated by coloured drawings. Preparations of the parts operated on were also exhibited. The author concludes that, when the divided ends of the tendon are held in apposition and the limb is kept at rest, reunion will take place without leaving a cicatrix, but that when extension is made, the new material becomes organized, and persists as a permanent structure.

III. "On the Curvature of the Indian Arc" By the Venerable John H. Pratt, M.A, Archdeacon of Calcutta Communicated by Professor Stokes, Sec. R.S. Received November 8, 1859.

(Abstract)

In a paper published in the Philosophical Transactions for 1855, in which the author calculates the effect which the attraction of the mountain mass north of India has upon the plumb-line at stations in the plains on the south, he applied the deflections as corrections to the astronomical amplitudes, to ascertain what influence they would have upon the determination of the curvature of the Indian Arc of Meridian. The method he adopted was to compare together the two measured arcs between Kaliana and Kalianpur, and between Kalianpur and Damargida The calculation brought out an ellipse of which the ellipticity is $\frac{1}{421}$. Colonel Everest had deduced by a comparison of the same arcs, but with uncorrected amplitudes, an ellipticity $\frac{1}{191\ 6}$ These two values are on opposite sides of the mean ellipticity for the whole earth, from which they differ by about $\frac{4}{7}$ths and $\frac{4}{7}$ths of the mean

In these calculations the ellipses are found which *exactly* accord with the measured lengths and the corrected latitudes in the one case, and in the other the latitudes uncorrected for deflection A more correct method has been followed by Captain A Clarke, R E, in the volume of the Ordnance Survey just published by Lieut Colonel James, R E Captain Clarke takes the latitudes of the three stations mentioned above, as corrected for mountain attraction by Archdeacon Pratt, and supposing the corrected latitudes as well as the elements of the mean ellipse subject to error, he determines, according to the method of least squares, the ellipse which best corresponds with the mean ellipse and with the three corrected latitudes The resulting ellipse depends of course upon the height attributed to the mean ellipse, which is left arbitrary, to be assigned at the end of the calculation In this way Captain Clarke shows that it is possible to obtain an ellipse which differs much less from the mean ellipse, and yet which makes the differences between the three latitudes calculated from the ellipse and the observed latitudes corrected for deflection, very small

Since this calculation was made by Captain Clarke, the author communicated to the Royal Society an approximate estimate of the effect of the deficiency of matter in the ocean south of Hindostan, on the plumb-line On seeing Captain Clarke's result, he felt anxious to ascertain what effect this new disturbing cause would have upon it The present paper contains a repetition of the calculations of Captain Clarke, with the additional corrections to the latitudes due to the defect of attraction of the ocean introduced into them. Capt Clarke's result, the author finds, is thereby improved, the ellipse obtained coming out somewhat nearer to the mean ellipse, while the errors in the latitudes, which already were very small, are still further reduced. The following are the values of the semi-axes a b (in feet) and of the reciprocal of the ellipticity corresponding,—I. to the mean ellipse, as determined by Captain Clarke; II. to the first of the two ellipses (corresponding to two different degrees of importance assigned to the mean ellipse) obtained by Captain Clarke by combining the mean ellipse with the Indian Arc, III. to the ellipse II. recalculated by the author, with the additional correction for ocean attraction introduced.

		a	b	b $a-b$
I	.	20926500	20855400	294
II.		20920328	20846522	283 7
III		20919988	20846981	286 55

The residual errors of latitude at Damargida, Kaliana, and Kalianpur, which in Captain Clarke's ellipse were +1″ 05, −0″ 95, +1″·20, are now reduced to +0″·93, −0″ 37, +0″ 74

In conclusion, the author calculates the distance of a point in the latitude of Kaliana from the centre of the earth in the three ellipses, and finds it to be near 7000 feet greater in the ellipses II and III than in the mean ellipse I That deviations to such an extent as this from the mean ellipse should actually occur he thinks likely enough, and he is not disposed to have recourse to some yet undiscovered cause to reconcile the Indian Arc with the mean ellipse The occurrence of marine fossils in mountains and elevated regions, shows that great changes of level of the land relatively to the water have actually taken place, and it seems unlikely that an extensive internal change in the state of the earth would cause an upheaval or depression of the land or the water alone, it might rather be expected that both would be affected, though unequally Hence the absolute change of distance of the land from the centre of the earth may have been much greater than the elevation relatively to the water, while the phenomena adduced indicate that even the latter must have been very great.

IV. "Comparison of some recently determined Refractive Indices with Theory." By the Rev Baden Powell, M.A , F R.S , F.G S , F R A.S., Savilian Professor of Geometry in the University of Oxford. Received November 17, 1859.

In a series of papers inserted in the Philosophical Transactions (1835, 1836, 1837), and afterwards, in a more correct and complete form, in my Treatise 'On the Undulatory Theory applied to the Dispersion of Light' (1841), I endeavoured to investigate the great problem of the explanation of the unequal refrangibility of light on the principles of the undulatory theory, as proposed by M Cauchy about 1830, by numerical comparison with the indices observed, more especially in cases of the most highly dispersive media then examined

The general result then arrived at was, that while the theory applied perfectly through an extensive range of media of low and moderate dispersive power, it did not apply well to those of higher, and to the highest in the scale (which of course formed the true test of the theory) it did not apply within any allowable limits of accuracy Since that time little has been done towards prosecuting the subject

In the *experimental* part of the inquiry, about 1849, I had observed the indices for a few new media*, but these were not high in the scale, yet though perhaps thus of little importance, I have now thought it as well to go through the calculation for them the results are of the same general character as just described

Soon after, finding that my friend, the Rev T P. Dale, F.R.A.S, was desirous to carry on some researches of this kind, I placed at his disposal the apparatus with which I had determined all my indices†

In 1850 that gentleman communicated to the Royal Astronomical Society a short general account of his observations‡ relative to some substances not very high in the scale

In 1858, Mr Dale, in conjunction with Dr. J. H. Gladstone, F.R S., presented to the Royal Society§ a valuable series of determinations, evincing highly interesting results relative to the change of refractive power in various substances under different temperatures.

None of these media being high in the scale, they have little bearing on the main object of my inquiries. In two cases (viz water and alcohol) the indices agree so closely with mine, that it was not worth while to recalculate them In two other cases I have carried out the numerical comparison, which affords a good agreement with the theory.

Very recently the same gentlemen have, however, published some

* See British Association Reports, 1850, Sect. Proc. p 14

† Described and figured, British Association Reports, 1839

‡ Notices, vol. xi p. 47

§ Phil Trans. 1858.

observations on several other media, especially phosphorus, a substance at the very summit of the scale, for which I had long been extremely desirous to obtain some determinations of indices*

Among these results only two sets are in a form in which they can be made available for comparison with theory These are the indices for the standard rays in bisulphide of carbon, and for solution of phosphorus in that medium, which I have now calculated theoretically

The results (given in the sequel) in both cases indicate discrepancies between theory and observation too great to be due to any reasonable allowance for error, and we are confirmed in the conclusion before arrived at, that, *for highly dispersive substances, the theory,* IN ITS PRESENT STATE, *is defective*

But these comparisons are all made by means of the same formula employed in my former researches, viz that derived from Cauchy's theory by Sir W R. Hamilton, which he communicated to me, and which I explained in a paper in the Philosophical Magazine†

Considering the unsatisfactory condition in which the question was left when tried by the test of the higher media in my former inquiries, it is a matter of some surprise that in the long interval since the publication of those results no mathematician has been induced to *revise the theory* Some criticisms indeed were advanced by Mr Earnshaw‡, and others by Prof Mosotti and the Abbé Moigno§, bearing on the general principle Sir W R Hamilton's formula in particular was founded on certain assumptions confessedly but *approximate* It remains then a promising field for inquiry to analysts, whether a better formula might not be deduced, or other improvements made in the general theory, by which a method applying so well to lower cases might be made equally successful for the higher

Results of calculation, for Ether, Hydrate of Phenyl, Oils of Spikenard, Lavender and Sandal-wood, Benzole, Bisulphide of Carbon, and Solution of Phosphorus in that medium.

Three indices assumed from observation, viz μ_B, μ_F, and μ_H, give the medium constants, viz.

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* See Phil. Mag July 1859

† Vol. viii N S. March 1836

‡ See Phil Mag. April 1842 and August 1842

§ See British Association Reports, 1849, Sect. Proc p 8

$$D = \mu_F - \mu_B,$$
$$D' = \mu_B + \mu_H - 2\mu_F$$

The values of the wave-length constants A and B for each ray, independent of the medium, are taken from my Treatise (Undulatory Theory applied to Dispersion, &c., Art. 270). Combining these, we obtain AD and BD′ for each ray in the medium.

Thence Sir W. R. Hamilton's formula (*ib.* Art. 237) gives, for any ray,

$$\mu = \mu_F \pm (AD + BD'),$$

the upper sign being used for rays above F, the lower for those below.

Ether.—Dale and Gladstone.

Ray	μ		Difference
	Observation	Theory	
B	1·3545		
C	1·3554	1·3544	− ·0010
D	1·3566	1·3566	− ·0000
E	1·3590	1·3586	− ·0004
F	1·3606		
G	1·3646	1·3646	·0000
H	1·3683		
Hydrate of Phenyl.—Dale and Gladstone.			
B	1·5416		
C	1·5433	1·5428	− ·0005
D	1·5488	1·5495	+ ·0007
E	1·5564	1·5567	+ ·0003
F	1·5639		
G	1·5763	1·5772	+ ·0009
H	1·5886		

In both these media, of low dispersive and refractive power, the accordances of theory and observation are sufficiently close.

Oil of Lavender.—Powell.

Ray	μ		Difference
	Observation	Theory	
B	1·4641		
C	1·4658	1·4632	− ·0026
D	1·4660	1·4678	+ ·0018
E	1·4728	1·4726	− ·0002
F	1·4760		
G	1·4837	1·4848	+ ·0011
H	1·4930 ?		

Oil of Sandal-wood.—Powell

B	1·5034		
C	1·5038	1·4988	− ·0070
D	1·5091	1·5062	− ·0029
E	1·5117	1·5102	− ·0015
F	1·5151		
G	1·5231	1·5271	+ ·0040
H	1·5398 ?		

Oil of Spikenard —Powell

B	1·4732		
C	1·4746	1·4744	− ·0002
D	1·4783	1·4082	− ·0001
E	1·4829	1·4826	− ·0003
F	1·4868		
G	1·4944	1·4945	+ ·0001
H	1·5009		

Benzole.—Powell.

B	1·4895		
C	1·4961	1·4907	− ·0054
D	1·4978	1·4965	− ·0013
E	1·5041	1·5029	− ·0012
F	1·5093		
G	1·5206	1·5210	+ ·0004
H	1·5310		

In oil of lavender and of sandal-wood there was some indistinctness in the line H which renders its index a little uncertain. It may be owing to this circumstance that the assumption of that index may have occasioned the discrepancy between theory and observation

In oil of spikenard the accordance is good In benzole the discrepancies are too great

Bisulphide of Carbon —Dale and Gladstone.

Ray	μ Observation	μ Theory	Difference
B	1·6177		
C	1·6209	1·6169	—·0040
D	1·6303	1·6251	—·0052
E	1·6434	1·6425	—·0009
F	1·6554		
G	1·6799	1·6807	+·0108
H	1·7035		

Phosphorus dissolved in Bisulphide of Carbon.—Dale and Gladstone.

B	1·9314		
C		1·9298	
D	1·9527	1·9522	—·0005
E	1·9744	1·9726	—·0018
F	1·9941		
G	2·0361	2·0363	+·0002
H	2·0746		

In the first of these media the differences are greater than can be fairly allowed to errors of observation

In the second case it is yet more clearly apparent that the theory is defective. The ray C was not observed, but the theoretical index is evidently in error to a large amount, as it is even lower than that of B The indices for D and C are perhaps within the limits of error, but that of E is too much in defect to be allowed.

December 22, 1859

SIR BENJAMIN C BRODIE, Bart., President, in the Chair.

Bennet Woodcroft, Esq, was admitted into the Society.

The following communications were read —

I "On the Electric Conducting Power of Alloys" By A. MATTHIESSEN, PH D Communicated by Prof WHEATSTONE Received November 17, 1859.

(Abstract.)

In this paper I have given the determinations of the electric conducting power of upwards of 200 alloys, and have found that the metals employed may be divided into two classes, viz —

A. Those metals which, when alloyed with each other, conduct electricity in the ratio of their relative volumes

B Those metals which, when alloyed with one of class A, or with each other, *do not* conduct electricity in the ratio of their relative volumes, but *always less.*

The alloys may be divided into three groups, viz —

1. Those made of the metals of class A with each other.
2. Those made of the metals of class A with those of class B
3. Those made of the metals of class B with each other.

From the experiments described in the paper I have tried to deduce the nature of alloys, and have arrived at the following conclusions :—

A. That most alloys are only a solution of one metal in the other, for,—

1. On looking at the curves belonging to the different groups, we see that each group of alloys has a curve of a distinct and separate form. Thus for the first we have nearly straight lines, for the second, the conducting power decreases always rapidly on the side of the metal belonging to class B, and then turning, goes almost in a straight line to the metal belonging to class A, for the third group we find a rapid decrement on both sides of the curve, and the turning-points united by almost a straight line.

2. On examining that part of the curve where the rapid decrement takes place, we find that with the lead and tin alloys it generally requires twice as many volumes of the former as of the latter to reduce a metal of class B to a certain conducting power

3 That the turning-points of these curves are not chemical combinations we may assume from the fact that they only contain very small per-centages of the one metal.

4 That the alloys at the turning-points have their calculated specific gravities.

5 From the similarity of the curves of the conducting power of alloys, where we may assume we have only a solution of one metal in the other, we may always draw approximatively the curve of the alloys of any two metals if we know to which class they belong

B That some alloys are chemical combinations; for—

1. At the turning-points of the curve we generally find contraction or expansion.

2 We have no regular form of curve, so that we cannot *a priori* approximatively draw it

3 At the turning-points of the curve, the alloy retains large percentages of each metal

4. The appearance (crystalline form, &c) of the alloys at these points is different from each other

C That some alloys are only mechanical mixtures, for example, bismuth-zinc, lead-zinc, some silver-copper alloys, &c.

The question now arises, To what is the rapid decrement of the conducting power of the metals belonging to class B, when alloyed with other metals, due?

The only answer I can at present give to this question is, that most of their other physical properties are altered in a like manner; for where we find no marked change in most of the physical properties, as in group I, and in the second group on the one side of the curve, then we have nearly their calculated conducting powers.

In the Appendix I have given some determinations of the conducting power of pure gold, which I find has a higher value than that generally quoted.

In conclusion, I may take this opportunity of thanking Dr. M. Holzmann for the excellent manner in which he has carried out the greater part of the experiments

II "On the Specific Gravity of Alloys." By A. MATTHIESSEN, Ph D Communicated by Professor WHEATSTONE Received November 17, 1859

This communication consists of a revised version of a Paper of the author, having the same title, which was read on the 19th May, and of which an abstract has been already given under that date (page 12)

III "On an extended Form of the Index Symbol in the Calculus of Operations." By WILLIAM SPOTTISWOODE, Esq, M.A., F.R.S. Received November 24, 1859.

(Abstract.)

In the case of two variables (the only one considered in the present paper), the term Index Symbol means the operation

$$\nabla = x\frac{d}{dx} + y\frac{d}{dy}$$

The new symbol is

$$\nabla_1 = y\frac{d}{dx} + x\frac{d}{dy}$$

The symbols Ξ, Ξ_1 have the following meaning,

$$\Xi = x\frac{d'}{dx} + y\frac{d'}{dy},$$

$$\Xi = y\frac{d'}{dx} + x\frac{d'}{dy},$$

where the accent indicates that in the combinations of Ξ, Ξ_1 the differentiations are to affect the subject of operation alone, and not x or y, so far as they appear explicit in the values of Ξ, Ξ_1. The first object of the paper is to develope the relations between the combinations of Ξ, Ξ_1 and ∇, ∇_1, and it is found that

$$\Xi_1{}^{\iota} = \begin{vmatrix} \nabla_1 & \iota-1 & 0 & . & 0 \\ \nabla & \nabla_1 & \iota-2 & . & 0 \\ \nabla_1 & \nabla & \nabla_1 & & 0 \\ & & & . & \\ \nabla & \nabla_1 & \nabla & . & \nabla_1 \end{vmatrix}, \text{ or } = \begin{vmatrix} \nabla_1 & \iota-1 & 0 & . & 0 \\ \nabla & \nabla_1 & \iota-2 & .. & 0 \\ \nabla_1 & \nabla & \nabla_1 & . & 0 \\ . & . & . & ... & . \\ \nabla & \nabla & \nabla & .. & \nabla \end{vmatrix}$$

according as ι is even or odd.

Again,—

$$(a, b, \ldots)(x, x_1)^n = (\alpha_n x + \beta x_1) \quad (\alpha_2 x + \beta_2 x)(\alpha_1 x + \beta_1 x)$$

$$= \begin{vmatrix} (\nabla\nabla_1)(\alpha_n\beta_n) & (\alpha_{n-1}\beta_{n-1}) & \\ (\nabla\nabla_1 \quad)(\alpha_n\beta_n)(& (\nabla\nabla_1)(\alpha_{n+1}\beta_{n-1}) & \ldots \\ \cdot & & \cdot \end{vmatrix}$$

where $(\nabla\nabla_1\nabla)(\alpha_2\beta_2)(\times)(\alpha_1\beta_1) = (\nabla\nabla_1\nabla)(\alpha_2\beta_2)(\alpha_1\beta_1)$

It is further shown that the effect of the operations ∇, ∇_1 on a given function, $u = \Sigma a_i x^{n-i} y^i$, may be represented by

$$F(\nabla, \nabla_1)\,u = \Sigma\left\{F\left(u, (n-i+1)\,\epsilon^{-\frac{d}{di}} + (i+1)\,\epsilon^{\frac{d}{di}}\right)\right\} a_i x^{n-i} y^i;$$

and the case of $\dfrac{u}{F(\nabla, \nabla_1)}$ is examined in detail

The value of

$$s_1 s_2 \quad s_i \frac{d^i}{dx^{i-j}\,dy^j}$$

in terms of $s_1\frac{d}{dx}, s_2\frac{d}{dx}, \quad s_1\frac{d}{dy}, s_2\frac{d}{dy}, \ldots$ are calculated, (1) when $s_1, s_2, \ldots$ are any linear functions of x, y, (2) when they are any functions whatever; and, in case (1), the effect of the above operation on a given function is determined

IV. "Problem on the Divisibility of Numbers." By FRANÇOIS ELEFANTI, Esq. Communicated by ARTHUR CAYLEY, Esq. Received November 24, 1859.

Problem. To find a proceeding by which the divisibility of a proposed integer N by 7 or 13, or by both 7 and 13, may be determined through the same rule.

Solution. We can designate the number N by *abcd* . *mn*, so that (a) be the first or highest, and (n) the last or lowest digit in it, therefore we may put

$$N = abcde \ldots mn.$$

1. Take the first digit, and having placed it below the fourth, make the subtraction in the usual way, thus

$$N'=\begin{matrix}bcde\,..\;mn\\ -a\end{matrix}$$

2. Take the first digit of N′, and having placed it below the fourth, effect the subtraction, thus

$$N''=\begin{matrix}cde\quad mn\\ -b\end{matrix}$$

3. Continue in the same way till you have effected the subtraction upon (n), and you will have the rule:

Is $N''' = l'm'n'$ divisible by 7 or 9, or by 7 and 9 at the same time? then is the number N divisible too by 7 or 13, or by 7 and 13 at the same time

Ex. I.

$$N=71491$$

$$N'=\begin{matrix}1491\\-7\end{matrix}=1421$$

$$N''=\begin{matrix}421\\-1\end{matrix}=420=7\ 60,$$

therefore N is divisible by 7

Ex. II

$$N=246571$$

$$N'=\begin{matrix}46571\\-2\end{matrix}=46371$$

$$N''=\begin{matrix}6371\\-4\end{matrix}=6331$$

$$N'''=\begin{matrix}331\\-6\end{matrix}=325=5^2\ 13,$$

the number N is divisible by 13

Ex. III.

N=1,1 8 3 5 3 0 8 0 3
1,8 2 5
8,2 4 3
2,3 5 0
3,4 8 8
4,8 5 0
8,4 6 3
4 5 5=5·7 13,

the proposed number N is divisible both by 7 and by 13.

Ex. IV $N=7429$

$$N'=\begin{matrix}429\\-7\end{matrix}=422=2\cdot211,$$

the number N is divisible neither by 7 nor by 13.

Remark.—The above proceeding is but the application of a general principle, inherent to our decadic system The author is in possession of similar proceedings for all prime numbers up to 107, above that limit (except a few cases) the rules become more complicated, and lose the high value of easy application.

Taking again $N=abcde\ldots mn$, we have the following Table for operating by the different prime numbers up to 109 —

Divisors	Operation
7, 13	$d-a$
17, 59	$d-3a$
19, 53	$d-7a$
23, 43	$+\begin{matrix}c\,d\\a\,a\end{matrix}$
29	$e-5a$
31	$+\begin{matrix}d\\8a\end{matrix}$
37	$+\begin{matrix}d\\a\end{matrix}$
41, 61	$e-4a$
47, 71	$\begin{matrix}de\\-aa\end{matrix}$
67	$d-5a$
73, 137	$e-a$
79, 127	$\begin{matrix}de\\-3(aa)\end{matrix}$
83	$\begin{matrix}d\\+4a\end{matrix}$
89	$\begin{matrix}c & d\\+(2a), & a\end{matrix}$
97, 103	$\begin{matrix}e\\+9a\end{matrix}$
101	$c-a$
107	$c-7a$

Explanation —17, 59 $d-3a$ means, take three times the first digit, and subtract it from the preceding bcd

Ex gr I N=3,2 4 6 3 4 9 2 9 1

$$\begin{array}{r}
3{,}246349291\\
\underline{-9}\\
2{,}373\\
\underline{-6}\\
3{,}674\\
\underline{-9}\\
6{,}659\\
\underline{-18}\\
6{,}412\\
\underline{-18}\\
3{,}949\\
\underline{-9}\\
9{,}401\\
\underline{-27}\\
374
\end{array}$$

Note to *Ex* I

15= 17−2
18= 17+1
9= 17−8
21= 17+4
27=2 17−7

Therefore we can operate thus —

$$\begin{array}{r}
3{,}246349291\\
\underline{+8}\\
2{,}543\\
\underline{-6}\\
5{,}374\\
\underline{+2}\\
3{,}769\\
\underline{-9}\\
7{,}602\\
\underline{-4}\\
5{,}989\\
\underline{+2}\\
9{,}911\\
\underline{+7}\\
918
\end{array}$$

Hence the rule:—If a multiple of the digit to be subtracted be *below* the divisor, we can convert subtraction into addition. If it be beyond the divisor, we can subtract the excess instead of the multiple.

II. The symbol for 89 was $+{}^{c\,d}_{2a\,d}$ which means that the first ought to be placed below the fourth, and the double of the first below the third, thus :—

```
N=6,5 9 1 9 7 4 5 7
    +1 2 6
     -----
     7,1 7 9
        1 4 7
       -------
       3,2 6 7
           6 3
         -------
         3,3 0 4
             6 3
           -------
           3,6 7 5
               6 3
             -------
             7,3 8 7
                1 4 7
               -------
               5 3 4=6·89
```

But the work can be done more easily in the following way :—

$$a=\left\{\begin{array}{l}1\\2\\3\\4\;2aa=\\5\\6\\7\\8\\9\end{array}\right\}\quad\begin{array}{r}21\\42\\63\\84=-5\\16\\37\\58\\-10\\11\end{array}$$

Ex. gr. I.

```
N=6,5 9 1 9 7 4 5 7
      +3 7
      -------
      6,2 8 9
        +3 7
        -------
        3,2 6 7
            6 3
          -------
          3,3 0 4
              6 3
            -------
            3,6 7 5
                6 3
              -------
              7,3 8 7
                  5 8
                -------
                  4 5=5·89
```

II

$$\begin{array}{l}
N=997609099 \\
\quad\;\; +11 \\
\quad\;\; \overline{98\;\;0} \\
\qquad\;\; 11 \\
\qquad\; 8819 \\
\qquad\;\; -10 \\
\qquad\quad 8090 \\
\qquad\quad\; -10 \\
\qquad\qquad \overline{8099} \\
\qquad\qquad\;\; -10 \\
\qquad\qquad\quad \overline{89}
\end{array}$$

III If we search separately for 79 (neglecting for the while 127) we can proceed thus —

$$a=\left\{\begin{array}{ll}
1 & -33 \\
2 & +13 \\
3 & -20 \\
4 & +26 \\
5 & -\;7 \\
6 & -40 \\
7 & +\;6 \\
8 & -27 \\
9 & -60
\end{array}\right\}=-3(aa)$$

Ex 1

$$\begin{array}{l}
7955063=N \\
\quad\;\; +6 \\
\quad \overline{95566} \\
\qquad\; -60 \\
\qquad \overline{55063} \\
\qquad\quad\;\; -7 \\
\qquad\quad \overline{50560} \\
\qquad\qquad\; -7 \\
\qquad\qquad \overline{553}=7\;79
\end{array}$$

Ex. 2

2,2 7 0 8 9 7 0 2 2 4 7
2,7 2 1 9
7,2 3 2 7
2,3 3 3 0
3,3 4 3 2
3,4 1 2 2
4,1 0 2 4
1,0 5 0 7
4 7 4=6 79

Remark.—The principle which I have thus developed is touched upon in some manuals of arithmetic, when we are shown that the same remainder in the expressions

$$\mathrm{R}\left(\frac{1000}{7}\right)=\mathrm{R}\left(\frac{1000}{13}\right)=\mathrm{R}\left(\frac{1000}{11}\right)=-1$$

leads to the identity

$$7\ 11\ 13=1001$$

In the rules given in this Paper, I have shown the high *analytical* value of the principle; but the properties of numbers, to which we are led when we apply the same (principle) in a *synthetical* way, are not less remarkable. As an instance I may state that the symbol

$37 \ldots\ldots \overset{d}{\pm a}$ leads to a curious relation among the members 7, 11, and 37.

V. "On the Structure of the *Chorda Dorsalis* of the Plagiostomes and some other Fishes, and on the relation of its proper Sheath to the development of the Vertebræ." By Professor Albert Kölliker, of Wurzburg. Communicated by Dr. Sharpey, Sec. R.S. Received December 8, 1859.

I take the liberty to present to the Royal Society the results of an extended series of investigations into the development of the vertebræ of the plagiostomous and some other fishes.

I *Chorda dorsalis*

A. *Structure*

The chorda dorsalis of the Plagiostomes, of *Chimæra, Acipenser, Scaphirhynchus, Toxodon,* and *Lepidosiren,* shows *four* distinct parts, viz —

1st The *outer elastic membrane,* a homogeneous elastic coat, which is not unfrequently perforated with holes of different sizes, of the same kind as those of the fenestrated membrane of Henle

2nd The *proper sheath,* formed of connective tissue of fibrous appearance, and generally provided with many plasm-cells.

3rd The *inner elastic layer,* a reticulated elastic membrane, and

4th The *gelatinous substance* of the chorda itself, made up of soft cartilage-cells, of different sizes and generally provided with nuclei

Of these four layers it would seem that only the third and fourth are present in the higher animals, from the Amphibia (with the exception of the Batrachians) upwards, if, at least, my opinion be correct, that the structureless envelope of the chorda of these animals, generally called the sheath proper, corresponds to the third layer in the cartilaginous fishes On the other hand, it seems that many of the osseous fishes present the same complications of structure as the Plagiostomes, if it is true that the bodies of their vertebræ are developed from the proper sheath of the chorda. So, for instance, there exists a beautiful elastic internal layer outside of the remnants of the gelatinous chorda in the genus *Orthagoriscus.*

B *Form of the chorda proper.*

1st. The chorda retains in some instances its original cylindrical form, and this is the case when the vertebral column shows no indication of vertebral bodies (Cyclostomes, *Acipenser, Chimæra, Lepidosiren, Tilurus, Hyoprorus** (anterior vertebra)), as well as where vertebral divisions exist (*Leptocephalus, Helmichthys, Hyoprorus* (last vertebra)).

2nd. In other cases the chorda is contracted in the middle region of each vertebral body, which seldom happens where there is no trace of oscification (*Hexanchus*), but is very generally the case in ossified vertebræ (Squali, osseous fishes, perennibranchiate amphibia, *Cæciliæ*).

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* Two genera belonging to the Leptocephalidæ, described by me (see Kaup, Apodal Fishes of the British Museum London, 1856).

3rd Lastly, the chorda may be separated into as many parts as there are interstices between the vertebræ, which remaining parts in some cases are totally absorbed (*Raia* and most of the higher animals)

C *Anterior end of the chorda*

1st. In many full-grown fishes the chorda dorsalis reaches with its anterior attenuated end to the base of the cranium, and its cranial part is in some cases enveloped in its whole length by the cranial cartilage This fact has been long known with regard to the Acipenseridæ, Cyclostomi, and Sirenoidei, but the same thing occurs amongst the Squali, and has been observed by Stannius in *Prionodon*, and by me in *Heptanchus*, *Centrophorus*, *Acanthias*, and *Squatina* In these last fishes the chorda reaches as far as the region of the hypophysis, and is bent upwards at its termination, so that the end itself lies underneath the interior perichondrium of the cranium, or at least very near the surface of the cartilage In other cases only the hinder part of the chorda is enclosed by the cranial cartilage, whilst the anterior half lies in a groove at the under part of it, as in *Leptocephalus* and *Helmichthys* In one case (*Tilurus*) the whole cranial part of the chorda is free, and situated underneath the base of the cranium, between its cartilage and the perichondrium*.

2nd In some genera of Squali and most of the osseous fishes, the cranial part of the chorda is reduced to the anterior half of the first ligamentum intervertebrale.

3rd. In the genus *Chimæra*, the chorda ends in the foremost part of the vertebral column. In this case the connexion between the cranium and the column is maintained by an articulation, which on the side of the column is formed by the cartilaginous vertebral arches.

4th. In the Raiidæ, finally, the chorda ends at a greater distance from the skull, and in this case also the anterior part of the column, which is formed only by the coalesced arches, is connected with the cranium by a real articulation

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* In all these fishes there exists rather a strong connexion between the vertebral column and the cranium, in *Squatina* besides this there are two lateral articulations between the cartilaginous arches of the first vertebra and the lateral parts of the cranial cartilage

II. *Ossification and Development of the Bodies of the Vertebræ*

A. *General remarks on the part which the chorda takes in the formation of the vertebræ*

1st In all cases where the chorda ossifies, it is only its second layer, or the *sheath proper*, which undergoes changes. At the same time the elastica externa disappears totally, or is at least dissolved in such a manner that its remnants are scarcely distinguishable, whilst the elastica interna and the chorda proper generally remain unaltered In one case only, viz in *Scymnus lichia*, ossification is to be seen even in the gelatinous substance of the chorda

2nd The ossification of the sheath of the chorda has been observed as yet only in the Plagiostomes and in certain genera of the osseous fishes, but it very probably will be found in all osseous fishes On the contrary, it is absent in all higher Vertebrata—according to my observations, even amongst the Batrachia

B *Changes of the sheath of the chorda during ossification*

1. *Vertebral column*

1st. In the Plagiostomes the sheath of the chorda in the first place assumes a greater hardness in certain parts, these parts being transformed into fibro-cartilage or real cartilage, whilst the intervening parts retain their primitive softness In this manner the first indications appear of the vertebral bodies and intervertebral ligaments, the interior parts of which are formed by the chorda itself and the elastica interna. The histological changes going on during this formation of the vertebral bodies, viz the transformation of the primitive plasm cells of the sheath into cartilage-cells, and the development of the homogeneous interstitial substance of the cartilage out of the fibrous substance of the sheath, speak strongly in favour of the view that both kinds of cells and intervening substances are closely allied, whatever may have been the development of the elements of the primitive sheath.

In the *Leptocephali* the sheath of the chorda ossifies without having been transformed into cartilage, and the same seems to hold good for the other osseous fishes.

2nd. Whilst this transformation of certain parts of the sheath of the chorda into cartilaginous vertebral bodies is going on, there are also formed in the interior of each of these bodies peculiar vertical dissepiments These dissepiments, developed by an interior growth of the sheath of the chorda, whereby the chorda proper becomes constricted, occur in some cases in vertebræ without any or with very slight traces of ossification, as in *Hexanchus* and the anterior vertebra of *Heptanchus*, whilst they may be almost wanting in others pretty well ossified (*Leptocephalus, Helmichthys, Centrophorus*)

3rd The ossification of the cartilaginous vertebral bodies formed out of the sheath of the chorda never begins at the surface, but always in their interior, and also in their middle region, and is, as far as I know, without exception, in the first instance a calcified fibrocartilage, or what I call a fibrous bone

4th The first osseous parts have the form of thin *rings* (*Heptanchus*, anterior vertebra), which afterwards assume that of hollow and thin *double cones* (*Heptanchus*, posterior vertebra, *Centrophorus*)

5th. The growth of these double cones, which are the real osseous vertebral bodies, when once they have assumed their whole length, takes place especially at their *outer side*, through the addition of *calcified cartilage* (chondriform bone, Williamson, *Knorpel-Knochen* in German), which is formed from the outer chordal cartilage of the vertebral body In addition to this, the osseous double cone thickens also at the expense of the cartilage inside of it, but in a much smaller degree

6th. In some cases the outer growth is everywhere the same, and in this manner stronger double-coned vertebral bodies of uniform thickness are formed. In other cases the growth is in some parts more active than in others, and vertebral bodies then originate with outer ridges and lamellæ (*Heptanchus, Raia, Carcharias, Mustelus, Galeus*). In one single instance the ossification of the outer cartilage takes place in such a way that the exterior parts of the vertebral bodies are formed by alternating circles of chondriform bone and cartilage (*Squatina*)

7th. With regard to the extension of this growth of the vertebral bodies formed by the ossification of the sheath of the chorda, it is to be remarked, that in some cases the whole, or nearly the whole sheath of the chorda ossifies, as in *Squatina* and the Raidæ. In other cases greater or lesser parts of the primitive cartilage, inside and outside the vertebral body, remain in their primitive state (Squali)

2. *Skull*

In some instances even *the sheath of the cranial part of the chorda ossifies* in its hindermost part, and *forms a true vertebral body for the occipital vertebra,* which entirely corresponds to those of the column This has been observed by me as yet in *Leptocephalus* and several Squalidæ, but it is extremely probable that the *basilar occipital* of all osseous fishes, viz that part of this bone which resembles a common vertebral body, is developed quite in the same way

C *On the manner in which the outer ossifying layer is concerned in the formation of the bodies of the vertebræ*

1st In those cases where the outer ossifying layer, viz that layer in which the cartilaginous arches are developed, takes part in the formation of the vertebral bodies, there are to be distinguished two different processes,—one in which the crural cartilages themselves play a part in this formation, and a second, where only the periosteal layer between them is concerned

2nd Where the crural cartilages take a part, they form, in the first place, by their coalescence an *outer cartilaginous layer* around the body of the vertebra, which took its origin from the chorda, and which we shall henceforth call *the chordal vertebral body*

3rd. This outer cartilaginous layer ossifies in many cases, and this ossification may take place in *two* places only, viz on the right and left side of the vertebral body, as in *Heptanchus,* or in *four* places, in which case a superior point of ossification at the floor of the neural canal, and an inferior one at the roof of the hæmal canal, are added to the two lateral ones (*Acanthias, Scymnus*)

4th. These external ossifications of chondriform bone may retain their primitive form of plates, and may then be called the lateral, superior, and inferior osseous plates, or they acquire by additional growth, at the expense of the outer cartilaginous layer, the form of wedge-shaped or cuneiform bodies, and may be named the lateral, superior, and inferior wedges (*Zapfen, Keile,* Germ.)

5th. In both cases these external ossifications comport themselves in two different ways with regard to the chordal vertebral body, inasmuch as in some cases both coalesce at their ends (*Scymnus, Acanthias*), whilst in others they remain separated (*Heptanchus*).

6th In some peculiar cases (Squali, possessing a nictitating eyelid, viz. *Mustelus, Carcharias, Galeus, Sphyrna*) the cartilaginous arches remain separated, and then the intermediate periosteal layer performs the part of an osteogenic stratum The osseous parts produced in this way lie at the same places as the bony plates mentioned under 4 and 5, they always possess the form of wedges, and coalesce with the chordal vertebral body, in some cases only at their ends, in others in their whole length. Although these ossifications are not developed from cartilage and have a very peculiar structure—they consist of a calcified fibro-cartilage with peculiar ossified strong fibres running straight through their whole thickness, —it is clear enough that they exactly correspond to the above-mentioned plates and wedges of other Plagiostomes formed out of the coalesced crural cartilages

From certain modes of transformation of the sheath of the chorda, combined with certain changes of the outer ossifying layer, the following types in the composition of the vertebral bodies may be established

TYPE I.—*The vertebral body takes its origin entirely from the proper sheath of the chorda*

A *Sheath of the chorda thick.*

1st Vertebral bodies soft (fibro-cartilaginous), incompletely separated from each other, and only distinguished by the interior septa of the chorda *Hexanchus*

2nd Vertebral bodies partly cartilaginous, with annular ossifications of the form of short double cones Ligamenta intervertebralia very strong *Heptanchus* (anterior vertebra)

3rd Vertebral bodies wholly cartilaginous, with thin osseous double cones of good length in the middle of the cartilaginous body. *Centrophorus*

4th Vertebral bodies well ossified, cylindrical and strong, formed inside by strong osseous double cones, and outside by alternating layers of cartilage and bone. *Squatina*

B *Sheath of the chorda thin*

5th Vertebral body a thin hollow osseous cylinder, chorda proper in its whole length cylindrical. *Leptocephalus, Helmichthys, Hyoprorus* (last vertebra).

6th. Vertebral bodies slightly constricted osseous double cones, with external longitudinal ridges *Chauliodus, Stomias.*

TYPE II —*The vertebral body is formed partly from the sheath of the chorda and partly from the outer ossifying layer*

1st Chordal vertebral body partly cartilaginous, with a stronger osseous double cone in its middle part External part of the body formed by a thin layer of cartilage from the coalesced arches, with *two* lateral ossified plates. *Heptanchus* (posterior vertebræ).

2nd The same with *four* external ossifications, whose ends coalesce with the internal double cone *Acanthias, Scymnus.*

3rd Chordal vertebral body nearly totally ossified, of the form of a strong double cone, with strong external longitudinal ridges External part of the body a strong layer of cartilage with superficial ossifications continuous with those of the arches *Raia, Torpedo*

4th Chordal vertebral body nearly wholly ossified, of the form of a thick double cone External part of the body formed by cartilage, with four strong wedge-shaped ossifications uniting with the ends of the inner double cone *Scyllium*

5th. Chordal vertebral body a strong osseous double cone, partly with external ridges External part of the body formed by four strong, wedge-shaped ossifications, derived from the periosteal layer between the cartilaginous arches, which in some genera totally coalesce with the inner double cone, whilst in others this happens only at the ends of the latter *Mustelus, Carcharias, Sphyrna, Galeus*

TYPE III —*The vertebral bodies are wholly developed from the external ossifying layer.*

1st. The vertebral bodies are developed from four cartilaginous parts, viz. the superior and inferior arches. Anterior vertebræ of the Raiidæ

2nd The vertebral bodies are developed only from two cartilaginous or osseous parts

- *a.* From the two neural arches, which in uniting do not enclose the chorda, which lies underneath them. *Cultripes provincialis,* J. Muller, *Rana paradoxa,* Dugès
- *b.* From two lateral plates of ossified connective tissue, which

in uniting totally enclose the chorda. Acaudate Batrachia, according to my own observations

c From two lateral cartilages which enclose the chorda, and also develope the arches from themselves Higher Vertebrata.

In terminating this Note, I take the liberty of adding that the only information heretofore existing on the subject to which it refers, is that contained in the very valuable memoirs by J Muller* and Williamson† The part which each of these has contributed to the elucidation of this subject, will be stated in a paper which will appear in the next Number of the Wurzburg Transactions, to which I refer those who take a more special interest in this matter, and desire to know on what data the results here given are founded

VI "Remarks on the late Storms of October 25–26 and November 1, 1859." By Rear-Admiral FitzRoy, F R.S Received December 22, 1859

As many of our Society must doubtless be interested in the nature and character of that storm in which the 'Royal Charter' went to pieces on Anglesea Island, and as abundant information has been obtained from Lighthouses, Observatories, and numerous private observers, I would take this earliest opportunity of stating that the combined results of observations prove the storm of October 25th and 26th to have been a complete horizontal cyclone

Travelling bodily northward, the area of its sweep being scarcely 300 miles in diameter, its influence affected only the breadth of our own Islands (exclusive of the west of Ireland) and the coast of France

While the central portion was advancing northward, not uniformly but at an *average* rate of about twenty miles an hour, the actual velocity of the wind—circling (as against watch-hands) around a small central "lull"—was from forty to nearly eighty miles an hour.

At places north-westward of its centre, the wind appeared to "back" or "retrograde," shifting from east through north-east, and north to north-west, while at places eastward of its central passage, the apparent change, or veering, was from east, through south-east, south, south-west, and west.

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* Vergleichende Anatomie der Myxinoiden

† Phil Trans. 1850

Our Channel squadron, not far from the Eddystone, experienced a rapid, indeed almost a sudden shift of the wind from south-east to north-west, being at the time in, or near, the central lull, while, so near as at Guernsey, the wind veered round by south, regularly, without any lull. This sudden shift off the Eddystone occurred at about three (or soon after), and at nearly half-past five it took place near Reigate, westward of which the central lull passed

From this south-eastern part of England, the central portion of the storm moved northward and eastward Places on the east and north coasts of Scotland had strong easterly or northerly gales a day nearly later than the middle of England When the 'Royal Charter' was wrecked, Aberdeen and Banffshire were not disturbed by wind, but when it blew hardest, from east to north, on that exposed coast, the storm had abated or almost ceased in the Channel and on the south coast of Ireland.

Further details would be ill-timed now, but they will be given in a paper to the Royal Society, as soon as additional observations from the Continent, and from ships at sea, have been collected and duly combined with other records.

The storm of the 31st, and 1st of November, was similar in character, but its central part passed just to the west of Ireland's south-west coast, and thence north-eastward

Of both these gales the barometer and thermometer, besides other things, gave ample warning, and telegraphic notice might have been given in sufficient time from the southern ports to those of the eastern and northern coasts of our Islands

As it is the north-west half of the cyclone (from north-east to south-west, *true*) which is influenced chiefly by the cold, dry, heavy, and positively electrified polar atmospheric current, and the south-west half that shows effects of equatorial streams of air—warm, moist, light, and negatively electrified,—*places over which one part* of a cyclone passes are affected differently from others which are traversed by another part of the very same meteor, or atmospheric *eddy*, the eddy itself being caused by the meeting of very extensive bodies of air, moving in nearly, but not exactly opposite directions, one of which gradually overpowers, or combines with the other, after the rotation.

On the *polar* half of the cyclone, continually supplied from *that* side, the visible effect is a drying up and clearing of the air, with a rising barometer and falling thermometer, while on the equatorial side, overpowering quantities of warm moist air—rushing from comparatively inexhaustible tropical supplies—push towards the north-east as long as their impetus lasts (however originated), and are successively chilled, dried, and intermingled with the always resisting, though *at first* recoiling, polar current After such struggles these two currents unite in a *varying* intermediate state and *direction*, one or other prevailing gradually

Very plain and practical conclusions are deducible from these considerations —

One, and the most important, is that in a gale which seems likely to be near the central part of a storm, that should be (of course) avoided by a ship which has sea room : a seaman, facing the wind, knows that the centre is on his *right* hand in the northern hemisphere, on his *left* in the southern, he therefore is informed *how to steer*

Another valuable result is that telegraphic communication can give notice of a storm's approach, to places then some hundred miles distant, and *not otherwise forewarned*

The Society adjourned to January 12, 1860.

January 12, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The Right Hon. Edward Lord Stanley was admitted into the Society

The following communications were read.—

I "Notes of Researches on the Poly-Ammonias."—No VII. On the Diatomic Ammonias. By A W. Hofmann, LL.D, F.R S Received December 14, 1859

In continuing my inquiries into the nature of the organic bases, I was led in the commencement of the year 1858 to repeat some experiments on the action of dibromide of ethylene upon ammonia, which M Cloëz[*] had published in 1853. The repetition of these experiments compelled me to contest not only the formulæ of M Cloez, but also the general interpretation which he had given to his results

I have not hesitated to communicate my conclusions to the Royal Society[†].

M Cloëz[‡] shortly afterwards discussed my observations, and pointed out the arguments which induced him to maintain his formulæ and his interpretations

I have not replied to these remarks M. Cloez having stated in the same note that he was still engaged with his experiments and that his inquiry was nearly completed, I discontinued my experiments on the action of dibromide of ethylene upon ammonia, fully persuaded that the chemist, to whom we are indebted for the first observation of this reaction, in continuing his experiments would arrive at the same results which I had myself obtained

In discontinuing the discussion with M Cloez, I was not freed from the obligation of proving the general thesis of my note, viz the formation of diatomic bases by the action of diatomic bromides on ammonia I have given the proof in several communications[§] addressed during the last two years to the Royal Society, and especially in a note[||] describing some new derivatives of phenylamine and ethylamine published during last summer The formation of these bodies, their analysis and their transformations, have, I believe, settled the question at issue in a satisfactory manner

These researches have been the subject of some remarks on the part of M Cloez[¶], from which it appears that this chemist has interpreted my silence as a tacit admission of defeat, he rejects the formulæ which I have given for the diatomic derivatives of phenylamine and ethylamine, and blames me for having continued my researches on the diatomic bases without having previously replied to his observations.

Under these circumstances I have been compelled to resume the investigation of the action of dibromide of ethylene upon ammonia, and to reply, after nearly two years have elapsed without M. Cloëz's paper having been published, to the series of objections which this chemist has raised against the theory of the diatomic bases

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[*] L'Institut, 1853, p 213

[†] Proceedings, vol ix. p. 150

[‡] Comptes Rendus, xlvi. p 255.

[§] Proceedings, vol ix pp. 277, 287, 651.

[||] Proceedings, vol. x p. 104.

[¶] L'Institut, 1859, p. 233.

Since this continuation of my experiments throws considerable light upon this new class of compounds, I beg leave to submit them to the judgment of the Royal Society

In order to render more intelligible the line of argument which M Cloëz has brought forward against the diatomic notions, it will be useful to recapitulate in two words the subject of our controversy.

M Cloëz admits that in the action of dibromide of ethylene upon ammonia, the molecule of ethylene splits into radicals belonging to three distinct groups, viz the formic, acetic, and propionic series, these radicals acting upon *one* molecule of ammonia, in which each of them replaces one equivalent of hydrogen, give rise to the formation of three *primary monamines*, viz Formenamine, Acetenamine, and Propenamine

According to the view which I defend, the molecule of ethylene remains intact in the reaction, acting upon *two* molecules of ammonia in which 2, 4, or 6 equivalents of hydrogen are replaced respectively by 1, 2, or 3 diatomic molecules of ethylene, the dibromide of ethylene gives rise to the formation of three diamines belonging to the same family, a primary, a secondary, and a tertiary diamine.

Expressed in formulæ the two views may thus be represented —

Formulæ of M Cloëz.

Formenamine $C_2H_3N = \left.\begin{matrix}C_2H\\H\\H\end{matrix}\right\}N.$

Acetenamine $C_4H_5N = \left.\begin{matrix}C_4H_3\\H\\H\end{matrix}\right\}N.$

Propenamine $C_6H_7N = \left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N$

Formulæ of Dr. Hofmann

Ethylene-diamine $C_4H_8N_2 = \left.\begin{matrix}(C_4H_4)''\\H_2\\H_2\end{matrix}\right\}N_2$

Diethylene-diamine .. $C_8H_{10}N_2 = \left.\begin{matrix}(C_4H_4)''\\(C_4H_4)''\\H_2\end{matrix}\right\}N_2.$

Triethylene-diamine .. $C_{12}H_{12}N_2 = \left.\begin{matrix}(C_4H_4)''\\(C_4H_4)''\\(C_4H_4)''\end{matrix}\right\}N_2.$

It was by careful examination of the physical properties of the bases under consideration, and more especially by the absence of simple equations capable of explaining the formation of the first and of the third terms of the series, that I had first been led to doubt the correctness of M Cloez's formulæ, but I would not have expressed this doubt, if, on repeating the analysis of the first base, of formenamine, the slightest doubt on the subject had remained in my mind I did not at the time investigate the two other bases, and I limited myself to stating that the constitution of these bodies would probably be found analogous to that which I had experimentally established for the first term of the series

Let us now examine the objections which M Cloez has brought forward against my argument "*According to the hypothesis of M. Hofmann,*" says he, "*the action of ammonia on the chlorinated and brominated hydrocarbons cannot give rise to the formation of chloride or bromide of ammonium, the reaction consists simply in a combination of the two substances, without the separation of a third compound it is a case of symmorphosis or addition,*

$$(C_4H_4)Cl_2+2H_3N=(C_4H_4)H_4N_2,\ 2HCl$$

Experiment proves, however, that the reaction involves the elimination of hydrochloric acid and the fixation of the elements of amidogen apomorphosis and symmorphosis are accomplished side by side, as indicated by the following equation.—

$$C_4H_4Cl_2+2H_3N=(C_4H_3)H_2N,\ HCl+H_3N,\ HCl"$$

M Cloëz would be perfectly right if during the reaction no other base were formed except the first one. But he forgets altogether that in the process under examination—exactly as in the mutual reaction between bromide of ethyl and ammonia—several other bases of more advanced substitution are produced The equations which I give for the formation of these bodies likewise involve the elimination of bromide of ammonium, and in fact of considerable quantities of this compound

$$2(C_4H_4)''Br_2+4H_3N=(C_4H_4)_2''H_2N_2,\ 2HBr+2(H_3N,\ HBr)$$

$$3(C_4H_4)''Br_2+6H_3N=(C_4H_4)_3''N_2,\ 2HBr+4(H_3N,\ HBr).$$

The bromide of ammonium then, which separates in considerable quantity in the action of dibromide of ethylene upon ammonia, belongs to the second and third portions of the reaction; it has nothing whatever to do with the formation of the first base. M. Cloez, as I have pointed out, does not admit the simple equation which I have given for the formation of this body; he denies that it is simply formed by the union of the two compounds reacting upon each other. According to his opinion it is produced in a secondary reaction, occasioned by the intervention of heat. My experiments do not confirm this opinion. A mixture of dibromide of ethylene and alcoholic ammonia allowed to stand for some time at the ordinary temperature, deposited a quantity of crystals, from which I was enabled to extract, without distillation, simply by successive crystallizations, absolutely pure salt of ethylene-diamine, as proved by the analysis of the bromide, the chloride, and the platinum-salt.

In discussing the numbers which I have obtained in analysing the hydrate and the hydrochlorate of the first base, M. Cloez quotes the results on which he founds his own formula. A glance at these figures will show unmistakeably that they agree much better with my formula than with the one which he defends. The following are the analytical details of our analyses, together with the theoretical values required by each formula:—

	Formula of M. Cloez	Analysis of M. Cloez	Formula of Dr. Hofmann	Analysis of Dr. Hofmann
Carbon	31·58	31·12	30·76	30·67
Hydrogen	10·52	12·77	12·82	12·97

Every experimentalist has incontestably the first right of interpreting his analytical results; knowing, as he does, his methods, he will do it generally much better than any other person. In the case before us, however, I believe very few chemists would have interpreted the results of analysis as M. Cloëz has done. As far as I am concerned, I would always prefer to admit having lost 0·2 per cent. of hydrogen, to calculating a formula requiring 2·25 per cent. of hydrogen less than had been obtained by experiment. I would prefer this especially in analysing a substance like ethylene-diamine, attracting carbonic acid with the utmost avidity—a trace of which would very appreciably lower the experimental hydrogen—and containing so high a percentage of hydrogen, that the presence even of a small quantity of water would produce a somewhat similar effect.

The results which M Cloez has obtained in the analysis of the hydrochlorate are not less in favour of my views He finds 1·28 per cent. of hydrogen more than required by his formula, whilst admitting my theory, he would not have lost more than 0·13 per cent

I have since examined several other salts of ethylene-diamine, and the results fully confirm the conclusions drawn from my former analyses. It would be useless to quote these additional experiments, but I will mention the characteristic numbers furnished by the analysis of the anhydrous base, since the diminution of the equivalent exhibits in a more striking manner the differences between the theoretical values of the two formulæ. Ethylene-diamine retains the water with the greatest energy, and it is in fact only by protracted contact with metallic sodium that it is possible to obtain this body in the anhydrous condition. I give the numbers obtained by combustion, side by side with the theoretical values of the two formulæ ·

	Formula of M Cloez, C_2H_3N	Formula of Dr Hofmann, $C_4H_8N_2$	Analysis
Carbon	41·37	40·00	40·13
Hydrogen	10·34	13·33	13·31

These numbers require no commentary

It is not, however, in the results of analysis that M Cloez finds the chief support of his views, he quotes an observation which at the first glance appears fatal to the diatomic notions.

"*But there is,*" continues M Cloez, "*a capital fact* (un fait capital) *which completely settles the question at issue this is the vapour-density of the free base*"

This density has been found by experiment to be 1·42

"*The theoretical density calculated for my formula, referred to* 4 *volumes, is* 1·315, *the modified formula of M. Hofmann, likewise referred to* 4 *volumes, gives the theoretical density of* 2·699.

"*These results appear to me decisive, and I do not hesitate to maintain the formula of the new series of bases, of which I first pointed out the formation.*"

I entirely agree with M Cloez as to the importance of the determination of the vapour-densities, but I certainly arrive at a very different interpretation of his result.

In repeating the experiment of this able chemist, I have arrived, as might have been expected, at exactly the same number But this number refers to the *hydrated base*, and it is easily seen that the hydrated molecule, when in the state of vapour, must occupy 8 volumes In calculating the theoretical density corresponding to the diatomic formula when referred to 8 volumes, we arrive at the number 1·35, which coincides in fact with the number obtained by experiment.

It is obvious that under the influence of heat the hydrated base splits into anhydrous base (4 volumes) and water (4 volumes),

$$C_4H_{10}N_2O_2 = C_4H_8N_2 + 2HO,$$

and that, instead of taking the vapour-density of the intact hydrated molecule, M. Cloez has determined the density of a mixture of anhydrous base and water, which on cooling combined again, reproducing the hydrated compound. And here I must recall the observations of several chemists, especially those of M Bineau, of M Kekulé, and of M H. Saint-Claire Deville, each of whom has had the opportunity of explaining the anomalous vapour-densities in the transitory decomposition of the compounds submitted to experiment, and I would quote particularly a note by Professor Kopp*, in which this distinguished physicist has treated the question of anomalous vapour-densities in a general manner

In the case before us, there is a very simple experiment, calculated to remove all hypothesis from the above explanation,—the determination of the vapour-density of the *anhydrous base*

The experiment made with a substance the purity of which had previously been proved by analysis, led to the number 2·00, which indeed absolutely coincides with the theoretical density of the diatomic formula $C_4H_8N_2$ referred to 4 volumes. This theoretical density is 2·07, whilst the formula of M Cloez, likewise referred to 4 volumes, requires the theoretical density of 1·00.

The molecule of *ethylene-diamine* (formenamine) then, like those of all other well-examined organic compounds, corresponds to 4 volumes of vapour, and the vapour-density of the base, far from militating against the molecular value which I assign to this body, furnishes on the contrary an additional and incontestable argument in its favour.

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* Ann. de Chem et de Pharm. cv 390.

The preceding remarks are, I hope, sufficient to establish the formulæ of the diatomic ammonias upon a solid basis. I will therefore only briefly allude to some results which I have obtained in studying the products of decomposition of ethylene-diamine, and which are not less characteristic. Submitted to the action of nitrous acid, this base is decomposed with evolution of nitrogen; in the first stage of the reaction an indifferent crystalline body is produced, and the final result of the process is a large quantity of pure oxalic acid. The nitrogen evolved during the transformation is accompanied by a very volatile liquid, the odour of which is somewhat similar to that of aldehyde. At the time when I made these experiments I really believed the liquid to be aldehyde, but since I failed in obtaining the crystalline compound with ammonia and in transforming it into acetic acid, I abstained from mentioning this reaction in my note to the Royal Society. I have now scarcely a doubt that the volatile liquid was the oxide of ethylene, isomeric with aldehyde, since discovered by M. Wurtz. The transformation would be

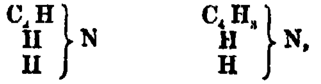
$$C_4H_8N_2+2NO_3=4N+C_4H_4O_2+4\,HO$$

In preparing the ethylene-diamine for my experiments, I obtained as a secondary product a small quantity of the second base, which M. Cloëz has described as acetenamine, and for which I now propose the term diethylene-diamine. This base has exactly the same percentage composition, whether viewed as a diamine or considered as the monatomic acetenamine of M. Cloëz. The analysis of the base itself, and of some of its salts, fully confirms the results obtained by that chemist. But this base is no primary monamine; it does not contain the radical acetyl, C_4H_3, as supposed by M. Cloëz; it is a secondary diamine containing two molecules of ethylene. Acetenamine, as conceived by M. Cloëz, should be formed by the action of chloride, bromide, and iodide of vinyl (C_4H_3Cl, C_4H_3Br, C_4H_3I) upon ammonia. These reactions do not furnish a trace of the base in question. But there is a more conclusive proof of the diatomic nature of this body, the evidence of which will not be contested by M. Cloëz,—this is the determination of the vapour-density. Experiment gave the number 2·7. The diatomic formula, $C_8H_{10}N_2$, referred to 4 volumes of vapour, requires 2·9. According to the monatomic view, a vapour-density of 1·45 should have been found.

The preceding experiments, although fixing in a satisfactory manner the composition and the equivalents of the two diammonias, do not unveil their molecular constitution—their degree of substitution

I have endeavoured to solve this problem by submitting them to the action of iodide of ethyl, a process which I have first used for similar purposes, and which has since become of general application This process, moreover, could not fail to furnish a final decision between the two theories

In considering with M Cloez the two bases as primary monamines belonging respectively to the formic and to the acetic groups,



$$\left.\begin{matrix}C_4H\\H\\H\end{matrix}\right\}N \qquad \left.\begin{matrix}C_4H_5\\H\\H\end{matrix}\right\}N,$$

it is evident that each of them must be capable of absorbing successively 1, 2, or 3 equivalents of ethyl, and of yielding *three* ethylated bases, two volatile, and one fixed On the contrary, if the bases were products of the successive substitution of the same molecule for the hydrogen of two equivalents of ammonia, if they were respectively a primary and a secondary diamine,

$$\left.\begin{matrix}(C_4H_4)''\\H_2\\H_2\end{matrix}\right\}N_2, \qquad \left.\begin{matrix}(C_4H_4)''\\(C_4H_4)''\\H_2\end{matrix}\right\}N_2,$$

the first of the two must likewise give rise to the formation of *three* bases, whilst the second one would produce only *two*

Experiment has verified this latter anticipation. In submitting ethylene-diamine to the alternate action of iodide of ethyl and oxide of silver, I have succeeded in obtaining two volatile ethylated bases, and a third one, which is fixed. These compounds are well defined, their composition was established by the analysis of their iodides or their platinum-salts Represented as salts, these bases contain—

Salt of ethylene-diammonium	$[(C_4H_4)''\ H_6N_2]''I_2$.
Salt of diethylated ethylene-diammonium	$[(C_4H_4)''(C_4H_5)_2H_4N_2]''I_2$.
Salt of tetrethylated ethylene-diammonium	$[(C_4H_4)''(C_4H_5)_4H_2N_2]''I_2$.
Salt of hexethylated ethylene-diammonium	$[(C_4H_4)''(C_4H_5)_6\ N_2]''I_2$.

On repeating the same experiments with diethylene-diamine, perfectly analogous phenomena were observed, but the reaction yielded only one volatile base, which was immediately converted into a fixed base Analysed in a similar manner, and represented as salts, these bases exhibit the following composition —

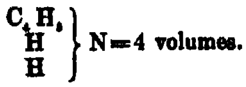
Salt of diethylene-diammonium	$\left[(C_4H_4)''_2 \quad H_4N_2\right]'' I_2$
Salt of diethylated diethylene-diammonium	$\left[(C_4H_4)''_2 (C_4H_5)_2 H_2N_2\right]'' I_2$.
Salt of tetrethylated diethylene-diammonium	$\left[(C_4H_4)''_2 (C_4H_5)_4 \quad N_2\right]'' I_2$

The same result is accomplished, but in a shorter and more elegant manner, by substituting iodide of methyl for its ethylated homologue Already, at an earlier period, I have shown that iodide of methyl has a remarkable tendency to yield the last product of substitution Thus, on treating iodide of methyl with ammonia, the iodide of tetramethylammonium is alone obtained, together with a very large *proportion of iodide of ammonium The action of iodide of methyl* with the ethylenated bases is perfectly analogous The last product of substitution is formed at once in notable quantity, and may be purified by a simple crystallization I have obtained in this manner, without being embarrassed by the intermediate compounds, the iodide of hexmethylated ethylene-diammonium and of tetramethylated diethylene-diammonium

These results require no further explanation

In the present state of science we rely upon a certain number of considerations which guide us in the construction of a chemical formula These are,—the study of the origin of a body, analysis, observation of the physical properties, and especially of the boiling-point, the determination of the vapour-density, and lastly, the examination of its metamorphoses I have endeavoured to look at the question under discussion from these several points of view, experiment has given invariably the same reply

It follows from this controversy that the diatomic alcohols imitate the monatomic alcohols in their deportment with ammonia Ethyl alcohol produces, as is well known, three ethylated ammonias, the molecules of which occupy 4 volumes of vapour.

Ethylamine 

$$\left.\begin{matrix} C_4H_5 \\ H \\ H \end{matrix}\right\} N = 4 \text{ volumes.}$$

$$\text{Diethylamine}\ \left.\begin{matrix}C_4H_5\\ C_4H_5\\ H\end{matrix}\right\}N = 4\text{ volumes}$$

$$\text{Triethylamine}\ \left.\begin{matrix}C_4H_5\\ C_4H_5\\ C_4H_5\end{matrix}\right\}N = 4\text{ volumes}$$

In a similar manner we find that glycol, the diatomic alcohol of ethylene, the discovery of which we owe to the remarkable labours of M. Wurtz, gives rise to three diatomic bases, corresponding to 2 molecules of ammonia, and representing likewise 4 volumes of vapour

$$\text{Ethylene-diamine}\ \left.\begin{matrix}(C_4H_4)''\\ H_2\\ H_2\end{matrix}\right\}N_2 = 4\text{ volumes}$$

$$\text{Diethylene-diamine}\ \left.\begin{matrix}(C_4H_4)''\\ (C_4H_4)''\\ H_2\end{matrix}\right\}N_2 = 4\text{ volumes}$$

$$\text{Triethylene-diamine}\ \left.\begin{matrix}(C_4H_4)''\\ (C_4H_4)''\\ (C_4H_4)''\end{matrix}\right\}N_2 = 4\text{ volumes}$$

The two first terms of this series are the bases which M Cloëz discovered about six years ago, but the true nature of which he failed to recognize To complete the series, it remains only to examine the third volatile base and the oxide of tetrethylene-diammonium

The observations which I have the honour of submitting to the Royal Society coincide in every point with the first note upon this subject which I presented nearly two years ago. I have simply carried out somewhat more in detail the sketch traced in my former communication

In conclusion I may state a fact which has also been observed by M Cloez, viz that the action of dibromide of ethylene upon ammonia gives rise to the formation of bases not directly belonging to the series which we have discussed In searching for the method of purifying the ethylene bases, I have been obliged to examine also the terms of the other group, but since these substances do not necessarily belong to this part of the inquiry, I omit for the present to enter more fully into their examination.

II "On the Forces that produce the great Currents of the Air and of the Ocean." By Thomas Hopkins, Esq Communicated by J P Joule, LL D Received December 2, 1859

(Abstract)

In this paper the writer pointed out the fact that we have at present no satisfactory evidence in books of what are the immediate causes of the great currents of the air and of the ocean, and he maintained that the liberated heat of condensing vapour is the cause of these currents He then proceeded to show that all the great winds terminate in comparative vacua created in particular localities where much vapour has been condensed, and contended that such vacua enable and cause heavier air to press and flow towards the parts which have been rendered light,—to re-establish the equilibrium of atmospheric pressure,—thus making heat the disturbing power in the aërial ocean, and leaving gravitation to act to restore an equilibrium. The great primary currents of the ocean were also described, and they were shown to be so situated as to be under the influence of the principal winds, which, in their passage over the waters, press on them, and force them forward as currents. These currents were maintained to be of a velocity, extent, and depth proportioned to the strength and continuity of the wind, showing that the pressure of the air on the water, whilst moving over it, is capable of producing the movement which takes place When, however, water is put into motion, it may be obstructed by land, and turned from its direct course, and in that way be made to form secondary currents But it was contended that heat of vapour, set free in the atmosphere, is the force which disturbs the equilibrium of pressure, and either directly or indirectly produces all the great continuous movements that take place both in the atmosphere and the ocean.

III. "On the Movements of Liquid Metals and Electrolytes in the Voltaic Circuit." By George Gore, Esq. Communicated by Professor Tyndall. Received December 1, 1859.

1. It has long been known that when a globule or layer of pure

and clear mercury is placed upon a smooth non-metallic surface, a watch-glass for example, and covered to a small depth with a watery electrolyte, sulphuric acid in particular, and two terminal platinum wires from a voltaic battery are dipped into the electrolyte, one on each side of the globule, the mercury makes a movement towards the negative wire, and a rapid and *continuous stream* of the supernatant liquid flows from the negative to the positive electrode over the surface of the mercury, and back again by the sides of the containing vessel Also that when a small drop of a watery electrolyte, especially sulphuric acid, is placed upon the surface of pure and dry mercury, the latter connected with the negative pole of a battery, and a platinum wire from the other pole momentarily immersed in the electrolyte, the drop of liquid is *suddenly repelled* and spreads over the surface of the mercury

2 These phenomena have been examined by Herschel*, Erman, H. Davy, Runge, Pfaff, and others†, and some of the results have been recorded in the 1st volume of 'Gmelin's Handbook of Chemistry,' page 486 (published by the Cavendish Society), but no definite cause of the movements seems to have been discovered Herschel has, however, shown that the continuous movement of the supernatant liquid is unaffected by the approach of strong magnets, and that it is influenced by the chemical nature of the electrolyte, also that its direction is notably influenced by the presence of various metallic impurities in the mercury.

3. Being desirous of ascertaining the conditions under which the movements are produced, the relations of the phenomena to ordinary and recognized actions, and the more immediate cause or causes of the movements, I have undertaken the following experimental investigation

4. In describing the experiments I shall have frequent occasion to speak of the continuous flow of the electrolyte, and of the sudden repulsion of drops of liquid already mentioned, and shall therefore speak of the former as the *continuous* action or movement, and of the latter as the *sudden* or *momentary* one. Also in speaking of the continuous motion, I shall call it *positive* flow or movement when the super-

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* "On certain Motions produced in Fluid Conductors when transmitting the Electric Current," Phil. Trans 1824.

† Draper has recorded some experiments of a similar kind.—Philosophical Magazine, S 3 vol. xxvi. p. 185.

natant liquid proceeds from the positive wire towards the negative one, and *negative* flow, &c. when it passes in the opposite direction.

5 The usual method of manipulation I have adopted has been to take a watch-glass of about 2 inches diameter and place in it by means of a small gutta-percha spoon capable of containing from 20 to 50 grains of mercury, a globule of that metal of about 30 grains weight, adding sufficient of the electrolyte to just cover or nearly cover the globule of metal, and sifting a few particles of finely powdered charcoal or asphaltum upon the surface of the liquid, to facilitate observation of the movements, next, using a Smee's battery of 22 pairs of plates 4 inches deep and 2½ inches wide with terminal platinum wires, charged with one measure of oil of vitriol and 15 measures of water, I place the end of the negative wire in the liquid about ¼th of an inch from the mercury, and then carefully immerse the end of the positive wire in the liquid on the opposite side of the globule, at a greater distance from the mercury than the negative wire in the case of an alkaline solution, and at a less distance in the case of an acid one, in order to prevent the mercury from touching the electrodes by its movement and thus vitiating the first and purest result A polished oval space 2 inches long, ¾ths of an inch wide, and ⅜ths of an inch deep, with a curved bottom, formed in a thick plate of glass and substituted for the watch-glass, did not admit of such satisfactory freedom of motion In doubtful cases of movement, a small porcelain boat, such as is used in organic chemical analysis, was sometimes employed instead of the watch-glass, and in certain special experiments a V-tube was employed In nearly all cases the mercury gradually became impure, and therefore fresh mercury was taken for each experiment.

A. *Conditions of the Movements*

6. *Two* substances are always required in these experiments, with one alone the movements never occur.

7. To determine whether *both* the substances must be in a *liquid* state:—1st. A portion of mercury in a watch-glass was connected with the negative pole of a battery and covered with a flat piece of platinum foil, a drop of solution of sulphate of potash was placed upon the foil and the end of the positive wire dipped into it. No movement, either sudden or continuous, of the solution or mercury took place. On substituting for the foil a circular piece of filtering paper varnished all round its edge and covered with several drops of the

solution of sulphate of potash, the sudden repulsions were produced readily, but were much less powerful than when the liquid was placed alone upon the mercury. 2nd Two circular clean spaces, ½ an inch wide, were scraped with a knife upon a horizontal plate of zinc, one of them was amalgamated with mercury and left covered with a very shallow layer of that metal, the other was also amalgamated, but the excess of mercury was wiped off, each of the spots was now covered with a shallow layer of a weak solution of sulphate of alumina, the zinc plate connected with the negative plate of the battery, and the end of the positive platinum wire dipped in succession into the supernatant portions of liquid, the solution above the thin layer of liquid mercury was powerfully repelled on making the contact, whilst that upon the other spot was unaffected Similar results were obtained with a solution of caustic potash, also with a plate of tin 3rd. A portion of Newton's fusible alloy was melted under a layer ⅛th of an inch deep of a solution of chloride of zinc, and the ends of the platinum wires from the battery immersed in the supernatant liquid until the alloy cooled and solidified, the zinc solution flowed from the negative towards the positive wire as long as the surface of the alloy remained in the liquid state, and ceased to flow immediately the surface of the metal solidified. Also a drop of a strong solution of caustic potash placed upon the melted fusible alloy, the latter connected with the negative pole and the former with the positive pole, exhibited the usual momentary repulsions as long as the surface of the alloy remained fluid. I therefore conclude that *both* the substances must be in a *liquid* state

8. To ascertain whether *both* the substances must be *conductors of electricity* —1st. I formed melted globules of phosphorus in warm oil of vitriol, also in a hot mixture of one measure of distilled water and two measures of oil of vitriol, and immersed the wires in the usual manner, but no motion of the liquid occurred. 2nd. No movements were obtained with a globule of bromine under warm oil of vitriol, a large globule of bromine was placed in a porcelain boat, and dilute sulphuric acid added until the bromine was partly covered, the wires were then applied, but no movements took place. Also the addition of sulphur and of selenium to the bromine did not ensure a different effect 3rd. With a large globule of selenium under fused chloride of zinc no motion was obtained. 4th. I made similar experiments with globules of chloroform, also of bisulphide of carbon in dilute sul-

phuric acid, but obtained no movements. 5th No movements took place with globules of chloroform in a solution of caustic potash or of sulphate of alumina.

9 To determine whether one of the substances must be *metallic* — 1st A definite layer of oil of vitriol was placed beneath a layer of distilled water weakly acidulated with sulphuric acid, and the terminal wires immersed in the upper liquid, no movements occurred at the boundary line of the two liquids. 2nd A dense solution of cyanide of potassium was placed in a small glass beaker, a few particles of charcoal were sifted upon its surface, and a layer of aqueous ammonia ½ an inch deep carefully poured upon it A vertical diaphragm of thin sheet gutta percha was then fixed so as completely to divide the upper liquid into two equal parts, the vessel was placed in a strong light, and two horizontal platinum wire electrodes from 66 pairs of freshly charged Smee's batteries immersed ⅛th of an inch deep in the liquid ammonia on each side of the diaphragm A copious current of electricity circulated, but no movements of the liquids at their mutual boundary line could be detected A small globule of mercury placed in the lower liquid at once produced evident signs of motion One of the substances must therefore be a *metallic* conductor of electricity.

10 To ascertain whether the capability of producing these movements was a general property of metals and alloys when in the liquid state:—1st. Bismuth was fused beneath a layer of chloride of zinc, tin was also melted under a similar layer, and the terminal wires immersed in the supernatant liquid, a steady negative flow occurred in each case 2nd. Cadmium was similarly treated under fused cyanide of potassium, and a positive flow obtained 3rd Cadmium, lead, Britannia-metal, and fusible metal were melted separately, small pieces of cyanide of potassium placed upon them and melted, the metal connected with the negative platinum wire, and the positive wire dipped into the melted cyanide; positive repulsions took place with each metal on making contact. I conclude from these experiments that the power of rotating under the influence of an electrolytic current is a general property of metals and alloys when in a liquid state.

11 That the *mass* or *body* of the metal is not essential to the production of the movements, is evident from the fact that the movements have been readily obtained with *thin layers* of mercury upon amalgamated zinc (7) and copper plates

12 I have endeavoured to obtain the movements without the presence of an electrolyte, by passing an electric current through a small globule of zinc fused upon the surface of bismuth, but the ready mingling of the melted metals, and their rapid oxidation, prevented a reliable experiment being made

13. It has already been shown, in the instances of fused salts upon melted metals (10), that the presence of *water* is not a necessary condition of the phenomena

14 The power of producing the movements is a general property of electrolytes as well as of liquid metals, I have experimentally found it in the following classes of substances :—organic and inorganic acids, water; aqueous solutions of caustic alkalies*, alkaline carbonates, bicarbonates, borates, hypophosphites, phosphates, sulphides, hyposulphites, sulphites, sulphates, bisulphates, iodides, bromides, chlorides, chlorates, nitrates, and silicates, salts of alkaline earths and of alumina, salts of tungsten, molybdenum, chromium, uranium, manganese, arsenic, and of the malleable heavy metals; also with fused salts, aqueous solutions of organic *salts*, and solutions of salts in alcohol. The salts of tungsten, molybdenum, chromium, uranium, and manganese, generally gave the weakest and most variable results, whilst sulphuric acid and solutions of alkaline cyanides yielded very strong and definite movements In feeble cases of motion the globule of mercury should be placed in a narrow porcelain boat, and a strong solution of the substance added until the metal is only covered at its sides with the liquid, and for still greater sensitiveness, the experiment of placing a drop of the liquid upon the surface of the mercury should be adopted

15 The *mass* or *body* of the liquid is not essential to the movements, mere films of solution adhering to the under surface of a circular disc of brass, brought into contact with mercury under the influence of a voltaic current, exhibited the phenomenon readily.

16. To ascertain whether the current of electricity must pass from the electrolyte into the metal, or *vice versâ* :—1st. A layer of mercury was placed in a narrow glass beaker, upon it a shallow layer of chlo-

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* Herschel found no movements with solutions of caustic alkalies (*Vide* Gmelin's Handbook, vol. i. page 490); I have readily obtained them with pure mercury in solutions of pure alkalies by using strong solutions and a powerful electric current, and placing only a small quantity of the liquid above the mercury so as to produce the maximum of effect. Alkaline solutions in general act much more feebly han acids

roform, and above this, in one instance a dilute solution of sulphate of alumina, and in the other instance, asolution of caustic potash, and the wires from the battery dipped into the upper liquid, no movements at either of the contiguous surfaces occurred 2nd Similar experiments were made, substituting in one case a definite layer of oil of vitriol with a very dilute solution of sulphuric acid above it, and in the other case a dense solution of chloride of zinc with a very dilute solution of the same salt above it, for the chloroform and its supernatant liquid, in each case only feeble movements in the usual direction at the surface of the mercury occurred, the weakness of the movements was probably a consequence of the increased distance of the electrodes from the mercury 3rd The lower part of a V-tube of half an inch bore was just filled with mercury, and a small quantity of solution of cyanide of potassium poured into each leg, on dipping the polar wires, one into the solution of each leg, the saline liquid rapidly flowed from the positive to the negative leg until it was 1¼ inch high in that limb From these experiments I infer that the electric current must pass from the electrolyte into the metal, or *vice versâ*, and that the continuous movements are not results of any power *radiating* from the electrodes

17 It is not essential that the electric current should pass both into and out of the metallic globule by the electrolyte, with a globule of mercury in rather strong sulphuric acid and either of the polar wires immersed in the acid, the other wire being in contact with mercury, the movements occurred: also with the negative wire touching a globule of mercury in a solution either of cyanide of potassium or strong caustic potash and the positive wire in the liquid, movements were readily obtained.

18. To ascertain whether the electrodes were essential to the movements, I placed a large globule of mercury in the middle part of a slightly bent horizontal glass tube, 20 inches long and ½ an inch diameter, then filled the tube with a strong solution of cyanide of potassium, and immersed the polar wires a short distance in the liquid at each end, a strong positive flow of the solution over the surface of the mercury occurred, but no movements took place at the surfaces of the electrodes, except such as were produced by the evolution of gas. The electrodes evidently operate merely as conductors of the electricity, and are not, in an abstract sense, at all connected with the movements

19 Herschel has shown that the approach of strong magnets has no effect on the motions (*vide* Gmelin's Handbook, vol i. p 490), and I have also found that the movements are not electro-magnetic A watch-glass—containing in one instance a solution of cyanide of potassium, in a second instance a solution of hydrochlorate of ammonia, and in a third instance oil of vitriol,—was placed upon one of the poles of a vertical horse-shoe electro-magnet capable of sustaining about 100 pounds, and the end of a large soft-iron armature which rested upon the other pole brought near the glass. The polar wires from the Smee's battery of twenty-two pairs were now immersed in the electrolyte on each side of the globule, and the magnet connected with a separate battery of large surface. The direction of flow of the electrolyte was instantly changed to a circular one all round the glass, and was reversed by reversing the polarity of the magnet. In each case the direction of motion of the electrolyte corresponded with that of the electric current beneath it, i e. with a south pole beneath, the liquid moved in the same direction as the hands of a watch ,—this circular motion was evidently a case of ordinary electro-magnetic action, as it occurred equally well without the presence of a liquid metal in the electrolyte No real connexion of the magnetism with the movements under investigation was detected.

20. To ascertain whether the movements varied with the quantity of the electric current, I prepared a single series of sixty-six pairs of Smee's batteries, forty of which were charged with spring-water, and the remainder with a mixture of one measure of sulphuric acid and fifteen measures of water The movements obtained on applying the current from the whole series to very dilute sulphuric acid containing a globule of mercury, were much more feeble and the amount of electrolysis much less than when the current from the twenty-six strongly-charged pairs alone was applied. On substituting distilled water for the dilute acid, the movements were stronger and the electrolysis greater with the whole series than with the twenty-six pairs. In all cases the movements appear to be dependent upon the quantity of electricity circulating

21. It is highly probable, from the experiments just described, that the movements are intimately dependent upon electro-chemical action occurring at the surface of the liquid metal, especially as the amount of motion varies with the quantity of electricity which passes from the electrolyte into the metal, or *vice versâ*, and it would be very

desirable to obtain a negative proof of this by an experiment with a globule of one liquid metal in a bath of some other liquid metal, as already attempted (12)

22 With every liquid yet examined the movement of the liquid has invariably been attended by a simultaneous movement of the fluid metal, and the greater the movement of the liquid the greater was the movement of the metal, from which I infer that the movements of the two substances are mutually dependent

23. The results in general indicate that the *sudden* movements are of the same general character as the *continuous* ones, the effect in the former case being heightened by the concentration of the electric force within a small compass, together with the additional electric energy always displayed at the moment of making contact with a battery

24. The movements require for their production two substances (6), both these substances must be in a liquid state (7), and be conductors of electricity (8), one of them must be a metal or a metallic alloy (9), any metal or alloy will do (10), and only a mere film of it is essential (11), the other must be an electrolyte, and need not contain water (13): any electrolyte will do (14), and only a thin layer of it is requisite (15): the electric current must pass from the electrolyte into the metal, or *vice versâ* (16), but need not pass both into and out of it by the electrolyte (17), the electrodes are not essential (18), the movements are not electro-magnetic (19), they are dependent upon the quantity of the electric current (20), and are intimately connected with electro-chemical action (21), the movements of the metal and electrolyte are mutually dependent (22), and the momentary movements are of the same nature as the continuous ones (23)

25. The pure or abstract conditions of the production of the phenomena are,—a liquid metal (or alloy) in contact with a liquid electrolyte, and a quantity current of electricity passing between them

B. *Conditions of the* continuance *of the Movements.*

26. With regard to the *continuance* of the movements·—1st In some cases the metal becomes covered with an insoluble film, produced by ordinary chemical action of the liquid, which prevents the continuance of the action, this occurs particularly with mercury in strong solutions of sulphides, iodides, bromides, and chlorides.

2nd If the positive wire is connected with the mercury and the negative wire with the liquid previous to placing both the wires in the electrolyte, films are in nearly all cases instantly produced (but not with strong sulphuric acid) and interfere with further action, films are also frequently produced by a similar cause upon the end of the mercury nearest to the negative wire when *both* the wires are in the solution, and in many such cases the mercury creeps in a peculiar serpent-like form beneath the film towards the negative wire 3rd. In many instances the metallic globule becomes of a pasty consistence by absorbing substances deposited upon its surface by electrolysis, and the motion declines, this takes place particularly with mercury in solutions of salts of ammonia, baryta, strontia, magnesia, and lime, but most with those of magnesia and lime, and it occurs very rapidly if, instead of placing both the polar wires in the electrolyte, the negative wire is immersed in the globule of mercury It is evident from these facts, that it is essential to the continuance of the movements, that the particles composing the surface of the metallic globule should retain a sufficient degree of mobility to admit of free motion

27 The best method of obtaining a *continuous* movement is to place a globule of pure mercury in a watch-glass, barely cover it with dilute sulphuric (or nitric) acid, connect it with the negative platinum wire and the liquid with the positive platinum wire of a battery of sufficient power to produce a moderate flow without overheating the liquid ten small Smee's batteries are sufficient By this means I have obtained undiminished motion for upwards of six hours

C *Conditions of the* direction *of the Movements*

28. In speaking of the *direction* of the movements, I always mean those of the supernatant liquid, unless otherwise stated, because the true movements of the mercury are generally less easily detected than those of the electrolyte · the movements of the liquid are best observed by means of charcoal or asphaltum (5), and those of the mercury by the aid of a few parallel scratches upon the under surface of the watch-glass.

29. The directions of flow of the metal and liquid are intimately dependent upon each other, for in every instance the metal moves in an opposite direction to the electrolyte (see also 22), and in those cases

where two opposite flows of the liquid towards the centre of the vessel occur (*as with a solution of sulphate of potash*), the globule of mercury is elongated at both ends into a pointed shape, and its two ends point toward the two electrodes, its largest diameter being directly under the point of meeting of the flows of the solution, and its acutest apex under the strongest flow The relation between the metal and liquid is apparently of a dual or polar character, the movements of the two bodies being always opposite and equal. This mutual dependence of the motions explains the necessity of *both* the substances being in a liquid state (7)

30 With regard to the *direction* of the flows, there are *three* cases to be distinguished —1st The movements obtained by immersing the negative wire in the metal and the positive one in the electrolyte 2nd Those obtained whilst the positive wire is in the globule and the negative one *in the electrolyte* 3rd Those produced by immersing *both* the wires in the supernatant liquid with the globule between them

31. Upwards of 150 different liquids, including organic and inorganic acids, alkalies, salts of alkalies, earths and heavy metals, also organic salts, were examined by the first of these methods, and in almost every instance the flow of the supernatant liquid was *positive*, the clearest exception being with a solution of persulphate of iron With concentrated sulphuric acid the motion (if any) was very feeble, but with diluted acid of various degrees of dilution it was strongly positive The flow of the liquid declined quickly with solutions which contained an alkali-metal, apparently in consequence of the mercury becoming less mobile (?) by absorption of that metal, but with dilute acids it continued a long time, with very dilute nitric acid, in one experiment the movement was sustained with scarcely any diminution during 2½ hours, and with dilute sulphuric acid, in a second experiment it continued 6½ hours, and did not then appear to slacken: the battery employed in this experiment consisted of ten small Smee's elements A globule of strong sulphuric acid was placed upon a surface of mercury, the latter connected with the negative pole, and the end of the positive wire dipped into the acid, much gas was evolved *from* the anode, and the liquid was not repelled on making contact, but collected in a heap around the wire;—a globule of solution of caustic potash

similarly treated exhibited repulsion on making contact It is evident from these uniform results that the direction of flow obtained by immersing the positive wire in the electrolyte and the negative one in the mercury is almost uniformly *positive*.

32. The movements obtained by this method are not produced by the act of deposited substances dissolving in the mercury, for they occur equally well whether hydrogen gas is set free and escapes or alkali-metal is deposited and dissolves in the mercury, until in the latter case the diminished mobility of the globule interferes with the result.

33 Considerable difficulty was experienced in examining liquids by the second method, in consequence of the rapid and in many cases instantaneous oxidation or filming of the metallic globule, but by using very dilute liquids and immersing the negative wire from seventy-two small Smee's elements during only a moment at a time, this difficulty was in most cases sufficiently overcome to allow distinct starts of the mercury to occur in the particular direction beneath its film, and thus to indicate an opposite motion of the supernatant liquid, although in nearly all cases the movement of the electrolyte itself could not be detected. Upwards of 100 liquids, consisting of organic and inorganic compounds—acid, alkaline, and neutral—were examined, and in more than three-fourths of them distinct movements of the metal were obtained, which were in every instance in a positive direction, thus indicating a *negative* flow of the electrolyte In some liquids, viz oil of vitriol, moderately dilute nitric acid, strong solutions of sulphate of ammonia, iodide of ammonium, and sulphite' of potash, very dilute solutions of bisulphate of potash, iodide of potassium, nitrate of cobalt, hydrocyanic acid, cyanide of potassium, and acetic acid,—visible movements of the liquid itself in a *negative* direction were also obtained. The movements of the liquid and of the metal very quickly ceased. These experiments show that the direction of flow obtained by placing the positive wire in the metal and the negative wire in the electrolyte is always *negative* •

34. The movements obtained both by methods 1 and 2 appear to be produced by a mutual attraction of the liquid and metal; in the former case the mercury attracts an electro-positive element of the liquid (hydrogen or an alkali-metal), and produces a positive flow;

and in the latter case it attracts an electro-negative element (generally oxygen), and produces a negative flow.

35 Herschel found by the third method of operating, that with pure mercury in acids and saline liquids the flow was negative, and was weaker as the base was stronger, and more rapid as the acid was stronger and more concentrated, and that in solutions of nitrates two opposite flows occurred, one from each wire (*vide* Gmelin's Handbook, i. 490) I have found by an examination of pure mercury in various liquids the results exhibited in the following Table The arrows indicate the direction of flow of the liquid, ← being positive and → negative, and the numbers affixed to them afford a rough approximation of the velocity or magnitude of the movements The battery employed consisted of twenty-two small Smee's elements The substances were dissolved in water, and the solutions were of moderate strength unless otherwise stated Manifestly impure substances were rejected, and fresh mercury was taken *for* each experiment. The results obtained were in many cases verified several times —

Distilled Water	→ faint	
Boracic Acid	$\rightarrow^{3}$	
Phosphoric Acid	$\rightarrow^{5}$	
Strong Sulphuric Acid	$\rightarrow^{10}$	
Strong Sulphuric Acid 1 measure Water 5 „	$\rightarrow^{5}$	
Strong Sulphuric Acid 1 measure Water 15 „	$\rightarrow^{5}$	
Strong Hydriodic Acid 1 measure Water 15 „	$\rightarrow^{6}$	
Hydrobromic Acid, very dilute	$\rightarrow^{2}$	
Strong Hydrochloric Acid 1 measure Water 5 „	$\rightarrow^{4}$	
Strong Hydrochloric Acid 1 measure Water 15 „	$\rightarrow^{3}$	
Perchloric Acid, very dilute	$\rightarrow^{4}$	$\leftarrow^{8}$
Strong Hydrofluoric Acid	$\rightarrow^{2}$	
Strong Hydrofluoric Acid 1 measure Water 5 „	$\rightarrow^{8}$	
Strong Nitric Acid, 1 measure Water 1 ,	$\rightarrow^{9}$	
Strong Nitric Acid, 1 measure Water 5 „	$\rightarrow^{8}$	
Strong Nitric Acid, 1 measure Water 15 „	$\rightarrow^{3}$	
Aqueous Ammonia, strong		$\leftarrow^{2}$
Sesquicarbonate of Ammonia		$\leftarrow^{4}$
Phosphate of Ammonia	$\rightarrow^{2}$	$\leftarrow^{5}$
Sulphide of Ammonium, 1 measure Water 15 „		$\leftarrow^{8}$
Sulphate of Ammonia	$\rightarrow^{1}$	$\leftarrow^{2}$
Hydrochlorate of Ammonia	$\rightarrow^{2}$	
Nitrate of Ammonia	$\rightarrow^{3}$	$\leftarrow^{2}$
Caustic Potash		$\leftarrow^{3}$

Carbonate of Potash	$-+^{1}$	$+-^{6}$
Bicarbonate of Potash	$-+^{4}$	$+-^{2}$
Sulphide of Potassium, dilute		$+-^{1}$
Sulphite of Potash	$-+^{2}$	$+-^{1}$
Sulphate of Potash	$-+^{3}$	$+-^{2}$
Bisulphate of Potash	$-+^{3}$	$+-^{1}$
Iodide of Potassium	$-+^{3}$	$+-^{3}$
Bromide of Potassium	$-+^{2}$	$+-^{3}$
Chloride of Potassium	$-+^{4}$	
Chlorate of Potash	$-+^{3}$	$+-^{2}$
Nitrate of Potash	$-+^{3}$	$+-^{2}$
Caustic Soda		$+-^{4}$
Carbonate of Soda	$-+^{1}$	$+-^{4}$
Bicarbonate of Soda	$-+^{2}$	$+-^{3}$
Biborate of Soda		$+-$
Diphosphate of Soda	$-+^{2\frac{1}{2}}$	$+-^{3}$
Sulphide of Sodium, dilute		$+-^{2}$
Hyposulphite of Soda	$-+^{\frac{1}{4}}$	$+-^{3}$
Sulphite of Soda		$+-^{5}$
Sulphate of Soda $-+^{4}$ and then	$-+^{4}$	$+-^{8}$
Chloride of Sodium	$-+^{4}$	
Nitrate of Soda	$-+^{3}$	$+-^{2}$
Phosphate of Soda and Ammonia	$-+^{\frac{1}{4}}$	$+-^{8}$
Baryta Water		$+-^{2}$
Carbonate of Baryta	$-+^{2}$	$+-^{1\frac{1}{2}}$
Chloride of Barium	$-+^{2}$	
Nitrate of Baryta	$-+^{3}$	$+-^{2}$
Strontia Water		$+-^{6}$
Chloride of Strontium	$-+^{2}$	
Nitrate of Strontia	$-+$	
Sulphate of Magnesia	$-+^{3}$	$+-^{1}$
Chloride of Magnesium, strong	$-+$	
Chloride of Magnesium, weak		$+-$
Nitrate of Magnesia, strong		$+-$
Nitrate of Magnesia, weak	$-+^{2}$	
Lime Water		$+-^{1}$
Sulphate of Lime	$-+^{2}$	

Chloride of Calcium	$-+^{2}$	
Chloride of Calcium in Alcohol	$-+^{1}$	
Nitrate of Lime	$-+^{4}$	$+-^{2}$
Sulphate of Alumina	$-+^{3}$	$+-^{2}$
Potash Alum	$-+^{4}$	$+-^{1}$
Hydrofluosilicic Acid	$-+^{4}$	$+-^{3}$
Silicate of Potash		$+-^{4}$
Molybdate of Ammonia	$-+$	
Chloride of Chromium, very weak	$-+^{1}$	
Monochromate of Potash	$-+^{2}$	
Nitrate of Uranium	$-+^{2}$	
Sulphate of Manganese	$-+$	
Arsenic Acid	$-+^{2}$	
Arseniate of Ammonia	$-+^{3}$	$+-^{5}$
Fluoride of Antimony	$-+^{2}$	
Antimoniate of Potash	$-+^{4}$	
Nitrate of Bismuth	$-+^{2}$	
Sulphate of Zinc	$-+^{4}$	$+-^{1}$
Iodide of Zinc, strong	$-+$	
Nitrate of Zinc	$-+^{3}$	
Iodide of Cadmium	$-+^{2}$	
Iodide of Tin, strong	$-+^{0}$	
Nitrate of Lead	$-+^{3}$	
Protosulphate of Iron	$-+^{4}$	$+-^{2}$
Persulphate of Iron	$-+$	
Chloride of Cobalt, weak	$-+$	
Nitrate of Cobalt	$-+^{3}$	
Sulphate of Nickel	$-+^{4}$	
Nitrate of Nickel	$-+^{5}$	
Sulphate of Copper, weak	$-+$	
Chloride of Copper, very weak	$-+$	
Nitrate of Copper	$-+^{3}$	
Nitrate of Mercury	$-+^{1}$	
Strong Aqueous Hydrocyanic Acid		$+-^{2}$
Strong Aqueous Hydrocyanic Acid, 4 measures } Aqueous Ammonia, 1 measure		$+-^{10}$
Cyanide of Potassium		$+-^{10}$

Strong Aqueous Hydrocyanic Acid, 5 measures Caustic Soda Solution, 1 measure		+−^{8}
Cyanide of Mercury	−+ faint	
Ferrocyanide of Potassium		+−^{4}
Sulphocyanide of Potassium	−+^{1}	+−^{3}
Oxalic Acid	−+^{2}	
Oxalate of Ammonia	−+^{3}	+−^{4}
Acid Oxalate of Potash	−+^{3}	
Neutral Oxalate of Potash	−+^{4}	+−^{8}
Formic Acid	−+^{3}	
Acetic Acid	−+^{3}	
Acetate of Potash	−+^{3}	+−^{1}
Acetate of Soda	−+^{3}	+−^{3}
Acetate of Baryta	−+^{2}	+−^{2}

Acetate of Uranium	−+ faint	
Acetate of Zinc	−+^{4}	
Acetate of Lead	−+^{1}	
Acetate of Copper	−+^{1}	
Tartaric Acid	−+^{5}	+−^{1}
Monotartrate of Potash	−+^{3}	+−"
Bitartrate of Potash	−+^{5}	
Bitartrate of Soda	−+^{3}	+−^{4}
Tartrate of Potash and Soda	−+^{3}	+−^{3}
Tartrate of Potash and Antimony	−+^{3}	
Citric Acid	−+^{4}	
Succinic Acid	−+^{4}	
Gallic Acid	−+^{2}	
Pyrogallic Acid	−+^{4}	
Carbazotic Acid	−+^{4}	
Benzoic Acid	−+^{2}	

Numerous interesting phenomena of motion and of colour, especially with solutions of salts of the earth-metals and with metallic iodides, were observed during the examination

36. On examining these numerous results we find:—1st That all alkalies and some alkaline salts produce a positive flow only 2nd That some alkaline and many neutral salts produce both positive and negative flows. 3rd. That some neutral and many acid salts, and nearly all acids, both organic and inorganic, produce a negative flow only. The stronger influence of acids, compared to that of alkalies (14, Note) in the production of these movements, is probably the reason why various salts of alkaline reaction give a negative as well as a positive flow, and why many neutral salts containing a strong acid (chlorides, for example) give a negative flow only No substance of alkaline reaction has been observed to give a negative flow only, nor any strongly acid substance to give only a positive flow. An *alkaline* or electro-positive substance as the electrolyte, produces therefore by the 3rd method a *positive* flow, and an acid or electro-negative substance produces a *negative* flow. Numerous analogies may be detected in the behaviour of similar salts on examining the Table.

37. The movements obtained by the 3rd method appear to be results of a similar mutual attraction of the mercury and the elements of the liquid to those obtained by methods 1 and 2 The mercury moves towards the cathode in acids because its positive end has acquired, by the aid of the electric current, a stronger affinity for the negative element of the liquid than its negative end has for the positive element, and moves towards the anode in alkalies because its negative end has acquired a stronger attraction for the positive element of the liquid than its positive end has for the negative element. *I do not, however, give either this or the previous explanation* (34) *as an ascertained fact, but merely as a temporary hypothesis to aid further investigation*

38 The amount of positive flow produced by the 3rd method in strong aqueous hydrocyanic acid, or strongest solution of ammonia, is comparatively small, apparently on account of their inferior electric conductivity , but if the smallest amount of ammonia is added to the hydrocyanic acid, the positive flow obtained is very strong , also, if instead of ammonia a small quantity of caustic potash, soda, baryta, strontia, magnesia, lime, or even alumina is added to the acid, similar effects are produced a little strontia or lime causes the nearest part of the mercury to dart up the watch-glass more than half an inch towards the positive electrode, if the battery is sufficiently strong. Silica had no effect. The addition of oxide of zinc, dioxide or protoxide of copper to the acid, reversed the direction of the flow, and dioxide of mercury neutralized the positive movement and diminished the conduction The strongest positive flows obtained by the 3rd method were with strong solutions of alkaline cyanides, and the strongest negative flows with sulphuric acid.

39. The behaviour of liquids upon mercury in V-tubes by the three methods is not essentially different from their behaviour in a watch-glass , the former, indeed, may be safely predicted from the latter. Sufficient pure mercury was placed in a V-tube of half an inch bore just to fill it at the bend, then a strong solution of sulphate of alumina poured upon it half an inch deep in each leg , on connecting the platinum wires from twenty-two pairs of small Smee's batteries with the solutions in the two legs, the liquid at once flowed from the *negative to the positive* leg , but by lowering the negative wire into the mercury, it flowed from the positive to the negative leg : no flow of the liquid was produced by placing the positive wire in

the mercury and the negative one in the solution of the negative leg. If the mercury was too deep to allow the liquid to pass, the solution insinuated itself down the sides of the mercury in the positive leg (the positive wire being in the solution, and the negative one in the mercury), but by using a suitable depth of mercury, the whole of the liquid flowed from the positive into the negative leg This is the usual behaviour of an *acid* liquid (or of one in which the negative flow of method 3 predominated) with a suitable quantity of mercury in a V-tube With a *strongly alkaline* liquid the only difference of behaviour is, that when the two wires are in the solutions of the two legs, the liquid flows from the *positive to the negative* limb (see 16), *i. e* opposite to the direction of flow with an acid.

40 There is a fixed relation between the direction of the electric current and the direction of each of the classes of movements, for in every case where the former is reversed, the latter also becomes reversed, but this effect is, of course, not observable in those cases of method 3 where two opposite and equal motions to the centre of the metallic globule exist.

41. I have examined the influence of the chemical nature of the metallic globule upon the movements obtained by the 1st method, in the following manner. The globule of mercury was first connected with the positive pole and the liquid with the negative pole for about ten seconds, and then the wires placed as in method 1; a *temporary negative* flow was produced for a few moments with certain liquids, apparently in consequence of the mercury absorbing a minute portion of an electro-negative constituent of the solution (?), and that substance causing a negative flow in the succeeding operation until the whole of it was redissolved. The following liquids exhibited this phenomenon of reversion —very dilute solutions of nitric acid, nitrates of ammonia, potash, soda, baryta, strontia (not of magnesia, apparently on account of viscosity of the mercury being produced), lime, zinc, lead, cobalt, nickel, copper, and dioxide of mercury; also sulphates of ammonia and potash; hypophosphite and diphosphate of soda, and, strongest, the alkaline nitrates;—but not dilute solutions of caustic potash, soda, baryta, or lime; carbonates or bicarbonates of potash or soda, carbonate of baryta, chlorides of ammonium, potassium, sodium, barium, strontium, magnesium, or

calcium; iodide or bromide of potassium, sulphites of potash or soda, biborate, hyposulphite, or sulphate of soda, sulphate of lime; arsenic acid, cyanide of potassium, oxalate of ammonia The battery used was a series of 72 small Smee's elements It appears from these experiments, that the direction of flow obtained by immersing the positive wire in the electrolyte and the negative one in the globule is strongly influenced by the chemical composition of the metallic globule

42. The chemical nature of the globule exercises an equally powerful influence upon the direction of the movements obtained by the second method If the mercury was first connected with the negative wire and the solution with the positive wire for a few seconds, and then the connexions reversed or made as in method 2, a *temporary* and strong *positive* flow of the electrolyte for a few moments was obtained, apparently in consequence of the mercury absorbing a little alkali-metal or other electro-positive constituent of the liquid, and that substance causing a positive flow of the solution until the whole of it was redissolved. This positive flow did not occur while there was above a certain quantity of the alkali-metal in the mercury. The reversions were obtained in the following liquids.—dilute and strong solutions of caustic potash, weak solutions of caustic soda, baryta, and lime, carbonate of baryta; chlorides of potassium, sodium, barium, strontium (not of magnesium, owing to viscosity of the globule), and calcium, iodide and bromide of potassium; sulphites of potash and soda; biborate, hyposulphite, and sulphate of soda, sulphate of lime; arsenic acid, cyanide of potassium; and oxalate of ammonia, also in solutions of hypophosphite and diphosphate of soda;—but not in very dilute nitric acid, nitrates of ammonia, potash, soda, baryta, strontia, magnesia, lime, uranium, zinc, cobalt, nickel, copper, or dioxide of mercury, sulphates of ammonia, potash, or alumina. It is worthy of notice that these two series are almost precisely the reverse of those named with method 1 (41); *i. e.* those liquids which have the property of reversing the flow of one method have not that property with the other method, except hypophosphite and diphosphate of soda. The explanation suggested (34), of the cause of the true movements of methods 1 and 2 does not appear applicable to these phenomena of reversion.

43. Herschel has shown that with pure mercury in solutions of

alkalies or of sulphate of soda (*vide* Gmelin's 'Handbook, i. 490, 492), if a little alkali-metal be introduced into the globule by connecting the latter for a few moments with the negative wire (the other wire being in the solution), a *positive* flow occurs on placing *both* the wires in the electrolyte with the mercury between them, and continues until all the alkali-metal is redissolved, and that similar effects are produced by adding small quantities of an easily oxidizable metal to the mercury—for example, potassium, sodium, barium, zinc, iron, tin, lead, or antimony, in the order given, but not by bismuth, copper, silver, or gold I have found that zinc added to mercury under a solution of sulphate of potash changed the direction of flow from positive and negative (obtained by method 3) to positive only, cadmium did the same, but more feebly, and tin still more feebly, bismuth had no apparent effect, but by using treble the electric power its effect was also similar, antimony also the same, gold had no apparent effect even with a current from 72 pairs of Smee's elements No positive flow was obtained by connecting the mercury with the negative wire and the solution with the positive wire for a short time in a liquid consisting of acid and water, and then placing both the wires in the electrolyte. Although there are many liquids (most of those which contain an alkali-metal) in which a temporary *positive* flow (or *increase* of positive flow) may be obtained by the 3rd method by first placing the *negative* wire in the mercury for a short time and then returning it to the electrolyte, there are but few (among which are diphosphate of soda and arseniate of ammonia) in which a temporary *negative* flow is produced by placing the *positive* wire in the mercury and then returning it to the solution It has been constantly observed with the 3rd method, that purity of the mercury is essential to the production of uniform results. From these various facts it appears that the chemical nature of the metallic globule strongly influences the direction of the movements obtained by method 3; also that an electro-positive globule produces a positive flow, and an electro-negative substance dissolved in the mercury produces a negative flow.

44 In some instances of the 3rd method—for example, with solutions of chloride of magnesium and nitrate of magnesia (35, Table), even the degree of dilution appears to determine the direction of the motion. No variation in the direction of the movement obtained by

either method was observed on varying the strength of the electric current, or on varying either the actual or relative distances of the electrodes from the metallic globule

45. The presence of an electro-positive metal in one portion of the surface of the mercury will (by generating a small electric current) sometimes cause rotation of the electrolyte after the battery-wires are removed, especially if the mercury is touched with a platinum wire beneath the surface of the liquid, this is seen most frequently with mercury into which some alkali-metal has been deposited

46 The general phenomena of the movements may be briefly redescribed thus ·—A When *both* the wires are in the electrolyte, and the mercury between them, several cases occur: 1 With a strongly *alkaline* liquid, a *positive* flow of the solution from the positive wire over the mercury to the negative wire occurs; 2 With a strongly *acid* liquid, a *negative* flow of the solution takes place, and 3. With a solution of a *neutral* or slightly alkaline salt, especially of a salt composed of a strong acid and a strong base, *two* flows occur, a negative one from the negative wire towards the centre of the mercury, and a positive one from the positive wire towards the centre of the mercury,—the negative one being generally the strongest. If in this 3rd case the mercury contains any impurity, or if a substance be caused by any means to dissolve in the mercury, the movements are notably affected an electro-positive substance (zinc, alkali-metal, &c) increases the positive flow so as partly or completely to overpower the negative movement, and an electro-negative substance increases the negative flow, in a few instances, so as to overpower the positive movement. These influences are also frequently detectable when liquids are used of alkaline or acid reaction, as in cases 1 and 2.

B When the negative wire is in the mercury and the positive one in the liquid, two cases occur· 1. With pure mercury, the motion is positive in nearly all liquids, whether acid, alkaline, or neutral; and 2. With mercury containing a small amount of an electro-negative substance, imparted to it by reversing the connexions of the wires for a short time, a temporary negative flow is produced in certain liquids, chiefly nitrates, but not in certain other liquids.

C. When the positive wire is in the mercury and the negative one in the liquid, also two cases occur. 1. With pure mercury, the motion is negative in all liquids—acid, alkaline, or neutral, and, 2. With mercury containing a *small* quantity of an electro-positive substance imparted to it by reversing the connexions of the wires for a few moments, a temporary and strong positive flow is produced in certain liquids and not in certain others—and these liquids are almost precisely the reverse of those named under B, 2.

The general influence of electro-positive substances dissolved in the *globule* is in all classes of cases to produce a positive flow, and of electro-negative substances to produce a negative motion, and the influence of electro-positive substances dissolved in the *liquid* is, in cases of A only, to produce a positive flow, and of electro-negative substances to produce a negative flow

47. The primary motions of the liquid and metal are, in all cases, wholly at their *surfaces of mutual contact*, whilst the movements observed are only secondary effects, useful in enabling us to infer the direction of the original motions. the *masses* of liquid and metal serve merely as conductors of the electricity, and as stores of material for supplying the acting surfaces The movements obtained are singularly symmetrical, probably in consequence of their essentially dual or polar character

48 The essential nature or principle of the movements appears to be *electro-chemical motion*, i. e definite motion directly produced by electro-chemical action.

49 To illustrate the action, I have constructed an apparatus consisting of two pairs of electrodes of platinum foil and mercury, suspended at opposite ends of two copper wires upon a central pivot, and rotating in an annular channel filled with dilute sulphuric acid, but the power was too feeble to produce revolution of the necessary moveable parts: it was not more than sufficient to produce a manifest tendency to motion.

In conclusion, I beg leave to suggest a trial of the sudden starts of the mercury by momentary currents as signals in electro-telegraphic apparatus

January 19, 1860

Sir BENJAMIN C BRODIE, Bart, President, in the Chair

The following communications were read.—

I "Abstract of a series of Papers and Notes concerning the Electric Discharge through Rarefied Gases and Vapours" By Professor PLÜCKER, of Bonn, For Memb R S

Received December 6, 1859

I *Action of the magnet on electric currents transmitted through tubes of any form*

The action exerted by a magnet on the luminous electric discharge, passing through a tube or any vessel of glass which contains residual traces of any gas or vapour, may be *generally* explained, if we regard the discharge as a bundle of elementary currents, which, under the influence of the magnet, change their form, as well as their position within the tube, *according to the well-known laws of electro-magnetic action.*

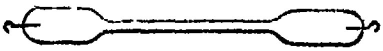
The concentration of the discharge into one free arch only takes place *if the arch be allowed to constitute a part of a line of magnetic force* [According to theory, there is no electro-magnetic action at all exerted on any element of a linear electric current which proceeds along such a line] This condition, for instance, is fulfilled in the case of an exhausted sphere of glass, through which the discharge is sent by means of two small apertures, if the sphere be put on the iron pieces of an electro-magnet in such a way that the two apertures coincide with any two points of a line of magnetic force.

There is another case of electro-magnetic equilibrium, which takes place *if the current proceed along an "epibolic curve," i e.* along a curve, falling within the interior surface of the vessel, whose elements, regarded as elements of an electric current, are perpendicular to the direction of the electro-magnetic force and impelled by this force towards the surface. An exhausted cylindrical tube, when equatorially placed on the iron pieces of the electro-magnet, presents the simplest instance of this case. All elementary currents are concentrated by the magnet along one straight line, which, according to the direction of the discharge and to the magnetic polarity, occupies either the highest or the lowest position within the tube When the axis of the tube makes an angle of 45° both with the axial and equatorial direction, the epibolic curve is found to be a fine spiral.

If neither of the two conditions mentioned above can be fulfilled, *i e* if the current cannot proceed either along a free magnetic or an epibolic curve, *no voltaic arch will be obtained, the current will be disturbed, and its light diffused* Thus, for instance, is the case when the above-mentioned cylindrical tube is placed axially on the two poles of the magnet there will be seen above each of these poles a luminous epibolic straight line, lying within the horizontal plane which passes through the axis of the tube, one on each side of this axis, but there exists neither a line of magnetic force nor an epibolic curve, joining the extremities of the two epibolic straight lines between the poles, hence diffusion of light

There are other classes of phenomena not at all indicated and explained by the laws of electro-magnetism In many cases *the magnet extinguishes the light of the current, without altering its intensity.* I sent the discharge of Ruhmkorff's apparatus at once through two exhausted tubes, communicating by a copper wire The first tube, about eight inches long and highly exhausted, was brought over the iron pieces of the electro-magnet into an equatorial position, while the second one was placed at some distance from the poles Whilst the magnet was not excited, both tubes became luminous by the transmitted current, when it was excited, the light of the first tube entirely disappeared, while the appearance of the second did not undergo the least change Hence we conclude that the disappearance of the light does not prove the extinction of the current.

Similar results are obtained when a tube having the shape of the annexed drawing is brought with its narrow middle part between the



two iron pieces. In this case the light disappears where the magnetic action is greatest, but not in the other parts of the apparatus. Sometimes before the light disappears, its colour is entirely changed, while in other cases, the magnetic force being less strong, only the change of colour takes place. The violet colour of sulphurous acid and of vapour of bromine is thus transformed into a fine green, and the blue of chloride of tin becomes a beautiful colour of pure gold.

If the magnet be strong enough, a phenomenon of the same kind is obtained when in the above-mentioned experiment the larger tube is put equatorially on the iron pieces, and the current concentrated in the lowest part of the tube Then, where the magnetic action is greatest, the illuminated epibolic straight line is interrupted, terminating in *two* cusps, which in the case of sulphurous acid show a green colour. Waving light goes from one cusp to the other In a room quite dark, highly diffused light is seen moving from one pole to the other, along lines of magnetic force.

When the sphere previously mentioned is placed on the iron pieces so that the two opposite apertures fall within the equatorial plane, the transmitted current will be concentrated along an epibolic curve, which in this case is one of the two arcs of the great circle within the equatorial plane, supplementary to each other, and joining the two apertures If it be the lower arc, turned towards the magnet, the epibolic current starting from the positive wire terminates in a cusp, where the action of the magnetic force, supposed generally to be strong, is greatest, while from the opposite side waving light enters the sphere In this case there is only *one* cusp

II. *The light of the negative wire bent by the magnet into curves and surfaces*

In order to render the new class of phenomena most brilliant and well defined, larger tubes of a cylindrical shape, into which long wires enter at both ends, are to be selected The light surrounding the negative wire must be as bright as possible, and the well-known dark space by which it is bounded must be very broad The magnet acts on this light in a peculiar way, having no analogy with phenomena hitherto observed. I easily discovered the law giving in all cases the exact description of the phenomenon

The light emanating from any not isolated point of the negative wire, and diverging in all directions towards the interior surface of the surrounding tube, *is bent by the action of the magnet into the magnetic curve, which passes through this point* According to the law already mentioned, such a curve is the only one along which an electric current can move without being disturbed by the magnet It equally represents the form which a chain of infinitely small iron needles, absolutely flexible and not subjected to gravity, would assume, if attached with one of its points in the point of the negative wire It is well known that a magnetic curve is completely determined by a single one of its points. Therefore the whole light, starting from all the different points of the negative wire, *will be concentrated within a surface generated by a variable magnetic curve.* The form of this "magnetic surface" varies according to the form of the negative wire, and its position with regard to the poles of the electro-magnet. When the negative wire lies within the equatorial plane, the magnetic surface assumes the shape of a vault, when the wire lies within the axial plane, the whole surface is contained within the same plane, and generally bounded by very well defined magnetic curves.

The negative light partly depends upon the substance of the wire Particles of it, either pure or combined with the included gas, are carried off to the interior surface of the tube, which, when platina wires are used, consequently is blackened If not acted upon by the magnet, all the part of the surface surrounding the platina wire becomes black, if acted upon, only that line along which the surface of the tube is intersected by the magnetic surface is blackened *In this case, therefore, the particles separated from the wire move along magnetic curves*

I think it most probable that the luminous electric currents in question are double currents,—going from the wire to the glass, and returning from the glass to the wire

The importance of the use of magnetic curves, or lines of magnetic force, in experimental researches, has been shown by several philosophers, especially by Mr Faraday Hitherto only filings of iron enabled us to give in peculiar cases an imperfect image of these curves. We may now *trace through space such a curve in the most distinct way and illuminate it with bright electric light.*

III. *The light of the positive wire and its spirals under the magnetic action.*

The origin of the current takes place at the positive wire If the negative wire is not too far from the positive, most striking phenomena are obtained when the magnet is acting on the formation of the electric current. In these experiments I made use principally of highly exhausted spheres, about two inches in diameter, through which two platina wires, either parallel or perpendicular to each other, were conducted, whose shortest distance was about 0 8 of an inch.

When, for instance, the sphere with *parallel* wires is put on the iron pieces of the electro-magnet so that both wires fall within the equatorial plane and are vertical, the whole circular section of the sphere passing through the negative wire is almost uniformly illuminated by violet light, while the light of the positive electrode appears at one of its extremities, whence it moves, along an epibolic curve, to the corresponding extremity of the negative wire. On reversing the polarity of the magnet, the illuminated epibolic curve passes from one extremity of the positive wire to the other, while the appearance of the negative light, after the reversion, is not at all altered.

When the positive wire terminates within the sphere, we get, according to the magnetic polarity, either an epibolic curve, or diffused light, starting from the free extremity of the positive wire towards the surface of the circle illuminated by the negative light.

When the sphere with *crossed* wires is put on the iron pieces so that the negative wire becomes vertical, the positive one horizontal and equatorial,—the whole surface of the axial circle passing through the vertical wire is illuminated, except the lower part, which is bounded by the magnetic curve starting from the lower extremity of this wire There is no light seen on the positive wire, which is intersected by the magnetic circular surface On this surface the shadow of the positive wire is most distinctly traced. [Shadows of this description are, in the general case, produced by beams of light starting from all points of the surface of the negative electrode, and moving along the corresponding magnetic curves. Not even the positive wire deviates such beams of light from their curved paths.] Nothing at all is changed by a change of magnetic polarity.

When the sphere is turned round its vertical diameter till the horizontal and positive wire passes from the equatorial direction into the axial, the whole surface of the axial circle passing through the negative wire is filled with illuminated magnetic curves. The positive light starts from the middle of the horizontal wire, and moves round it, within the equatorial plane, towards the negative wire without interfering with the light emanating from this wire. It constitutes a fine spiral, separated by a dark space from the positive wire, taking its origin in a cusp, and expanding more and more when it approaches to the negative wire In changing the polarity of the magnet, instead of the single spiral starting from the middle of the wire, we obtain two such spirals starting from the two extremities of the wire, impelled against the glass of the tube and revolving in the opposite sense.

The phenomena just described and all the various phenomena of the same class may be fully explained by the laws of electro-magnetic action. For this purpose, we admit that the first element of each elementary current starting from any point of the positive wire is directed towards the negative wire. This supposition follows from the observed fact, that the positive wire, if parallel to the negative, becomes luminous along its whole length on that side which is turned towards the negative wire When acted upon by the magnet, these first elements, bound to the positive wire, are allowed to move freely along this wire. The single point, or the system of points, where all the first elements meet before leaving the wire, may easily be determined The following elements, subjected to the same force, are entirely at liberty to obey it. This action may generally be defined thus —Imagine an element of the elementary electric current starting from any point of a magnetic curve, which connects both poles, imagine also the molecular currents (as assumed by Ampère) within the magnet continued round this magnetic curve Then, if the element be perpendicular to the magnetic curve, the full action of the magnet turns it round the curve in a direction opposite to the direction of Ampère's molecular currents. If it be not perpendicular, this action is to be decomposed along the normal to the magnetic curve within the osculating plane.

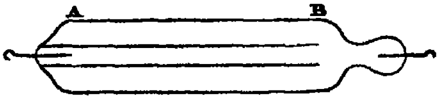
I think it worthy of notice, that the same electro-magnetic laws, applied to magnetic action on an already formed electric current, either bound to the conducting wire or free to move in space, equally hold in determining the path of the current when acted upon by the magnet during its formation between the two electrodes. Thus a moving electric particle, under magnetic action and tending towards the negative electrode, may be regarded as describing a curve, in an analogous way as a projected material point does—acted upon by gravity.

IV. *Electric currents returning on their own path*

When only one of the wires of Ruhmkorff's apparatus is in contact with one of the electrodes of a long and large exhausted tube, while the second wire is isolated, or, what is preferable, communicates with the earth, an electric current is obtained entering the tube and returning on its own path The existence of the two currents is most evidently proved by the magnet, which, acting differently on both, divides the double current into two of the same intensity. This intensity is much increased by touching the tube with the hand at any distance from the electrode which communicates with Ruhmkorff's apparatus

A double current of exactly the same description is obtained in a narrow tube, which may be one foot long, communicating with a larger tube, both electrodes of which are connected with the wires of Ruhmkorff's apparatus When the extremity of the narrow tube is touched with the hand, a current starting from the principal tube enters the narrow one, proceeds to its extremity, and finally returns to the principal tube: which is proved by the magnet.

When, through a tube whose shape is represented by the drawing,



the discharge was sent from A to B, the current, passing from the narrow tube into the surrounding larger one, was, when it arrived at the extremity of the narrow tube, partly branched off in an opposite direction towards A, and then, changing its course again, moved towards B.

There is another case where returning currents are obtained. When the discharge on its way through a highly exhausted tube passes from a large space into a narrow channel, a part of the current is reflected, and returns on its own path within the larger space: which is again proved by the magnet.

Mr. Gassiot first observed and described what he calls "reciprocating currents," and the separation of such currents by the magnet That celebrated philosopher had the kindness himself to show me his experiment, and even enabled me to repeat it, by obliging me with one of his fine Torricellian-vacuum tubes I do not exactly know his opinion on the nature of those currents, for my part, I think they are induced currents returning on their own path This opinion was supported by the subsequent experiments.

I got a hollow and highly exhausted sphere of glass about 2 inches in diameter, into which no wire entered, and placed it on the iron pieces of the electro-magnet On touching the sphere with one of the wires of Ruhmkorff's apparatus, diffused light was spread through it By the excited electro-magnet this diffused light was concentrated into a luminous stream, which moved along the magnetic curve determined by the point touched, from this point to another one in which the interior surface of the sphere is intersected a second time by that curve, and evidently returned in the same curve from the second point to the first When the point where the sphere is touched by the wire falls within the equatorial plane, no part of the magnetic curve in question, which in this case is a tangent to the sphere, enters it In this peculiar case, the phenomenon is entirely changed, the light of the induced current constitutes an ascending or descending stream proceeding along the epibolic curve within the equatorial plane In reversing either the polarity of the magnet or the direction of the discharge, the ascending stream is transformed into a descending one, or *vice versâ* But there is no change of the appearance in the general case

When the sphere is simultaneously touched by the two wires of Ruhmkorff's apparatus in two different points, we get within it two independent luminous currents showing no reciprocal action on each other, and it is only when both points belong either to the same magnetic curve or to the epibolic one, that there is but one luminous arch along that magnetic or the epibolic curve.

All this shows that Gassiot's induced streams are subject to the same laws as the direct currents are, formerly observed by myself

In all experiments mentioned in this section, the discharge of Ruhmkorff's apparatus may be replaced by a spark taken from the conductor of an electrical machine.

V. *Fluorescence produced by the electric discharge.*

Glass shows in sunlight scarcely any kind of fluorescence. A glass tube, including residual traces of a proper gas, becomes highly fluorescent when the electric discharge is sent through it: a fine green colour appears in the case of common German glass, a fine blue if the glass contain lead. Hitherto I have tried in vain to get specimens of a different and well-determined chemical composition, which, without doubt, would offer new and curious cases of fluorescence*

A beautiful fluorescence is obtained by surrounding suitable exhausted tubes, through which the discharge is passing, with a solution of æsculine, of fraxine, of sulphate of quinine, &c If the tubes include residual traces of hydrogen, scarcely any fluorescing light is seen, if they contain traces of nitrogen, on the other hand, the fluorescence is very intense All depends upon the nature and the density of the gas In certain cases, a very faint electric light, scarcely seen by the eye, produces a brilliant fluorescence, especially when the light, if belonging to the negative wire, is concentrated by the magnet M Geissler, in constructing his tubes, first observed (about eighteen months ago) the fluorescence of gases, which even continue luminous some seconds after the interruption of the current further researches will explain this curious phenomenon.

VI. *The spectra of the electric current in rarefied gases and vapours*

The light of the electric discharge through large tubes is rather too much diffused to give a distinct spectrum. I made trial, therefore, of sending the discharge through a capillary tube, and fully succeeded I got a brilliant luminous line within the tube, of which a beautiful spectrum was obtained by replacing the distant illuminated slit, which Fraunhofer used in his observations, by the self-luminous line Afterwards I employed Babinet's goniometer The slit of this instrument was illuminated by the current within the capillary tube, which was placed before it at a distance of about 0 4 of an inch The aperture of the slit could be changed; but where it is not particularly mentioned, it was seen under a constant angle of 3′. After having interposed the prism of heavy flint glass, *the refracted image of the slit, in the general case, was found to be divided into a less or greater number of differently coloured bands, appearing each under the just-mentioned constant angle of three minutes* Hence it follows that the analysed electric light is com-

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* From the beautiful experiments recently made by E. Becquerel, we may deduce a satisfactory explanation of these phenomena.

posed of a certain number of rays, whose refrangibility is a discontinuous one. The refraction of each ray is determined by the angle between the middle lines of the image of the slit, seen directly, and of the corresponding refracted band This angle is independent of the aperture of the slit, and remains the same if the slit be reduced to a physical straight line.

There cannot exist a deflected band smaller than 3′, *i e* no such band appears under an angle less than the angle under which the aperture is seen directly, without the interference of the prism This law holds through all my numerous observations of electric spectra [I find difficulty in applying it to the case of the common solar spectrum] There are, in many instances, bands observed which are seen under an angle greater than 3′, but generally such bands are resolved into two or more bands of single breadth In some instances where the angle in question does not reach the double of 3′, there appears in the middle part of the band a *bright* line, larger bands are generally divided by small, well-defined, *dark lines*

In order to distinguish the rays of different refrangibility in the different gases, I denoted such a ray by adding to the symbol belonging to the gas the Greek letters α, β, γ, indicating the succession of the corresponding bands in each spectrum Accordingly, for instance, the band $N\gamma$, appearing under an angle of 6′, is divided by a dark line into two, the bands $N\delta$ and $N\theta$, having each a breadth of 10′, are divided by two dark lines into three single bands The bands $Hg\alpha$ and $Sn\,Cl_2\gamma$, 5′ broad, show a bright middle line

The space between two bright bands is either absolutely black, or of a greyish tint, or of a faint tint of that colour which is indicated by the position of the space within the spectrum. With regard to these faint tints, the eye, by the effect of contrast, is commonly a very bad judge of colour, and there may easily be a deception, admitting a succession of colours in the spectrum which in reality does not exist.

I cannot enter here into the details by which I obtained exact measures of the minimum refraction of the different rays All measures were taken without deranging the adjustment of the goniometer, for ascertaining its constancy, each spectrum was compared with the spectrum of hydrogen. From the angles of refraction, I deduced the indices of refraction, and hence the corresponding lengths of waves expressed in millionths of a millimetre in order to get absolute numbers immediately comparable with those of others.

The discharge through a capillary tube containing residual traces of pure hydrogen is of a deep red colour, the spectrum obtained from it consists of only three bright bands:—of a most splendid red one, Hα, of a bluish-green one, Hβ, nearly as bright, and of a fainter violet one, Hγ. The following table contains the angle of refraction ϕ, the index of refraction μ, and the length of the wave λ, corresponding to each of the three rays Hα, Hβ, Hγ, as well as of the dark lines of Fraunhofer, C, F, G, in order to compare their reciprocal position —

	ϕ		
Hα	57° 10′ 5	C	57° 8′ 5
Hβ	59 55 5	F	59 55 5
Hγ	61 43	G	61 55 5

	μ		
Hα	1 7080	C	1 7077
Hβ	1 73255	F	1 73255
Hγ	1 7481	G	1 7498

	λ		
Hα	653 3	C	656 4
Hβ	484 3	F	484 3
Hγ	433 9	G	429 1

From this table it results that Hβ exactly coincides with F, while Hα and Hγ approach very near C and G.

The spectrum of pure oxygen was only obtained after several unsuccessful trials to procure proper tubes, the gas being in most cases absorbed by the electrode during the passage of the current.

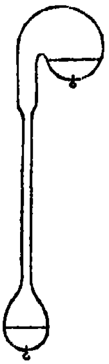
The spectrum of nitrogen is one of the richest in colours. Its less refrangible part is of a peculiar nature, having, from the exterior red to the limit of the yellow, seventeen equidistant small dark bands.

Most characteristic are the spectra of carbonic acid (oxide of carbon), iodine, bromium, chlorine, chloride of silicium, chloride of phosphorus, chloride of tin. The spectrum of the last-mentioned substance is one of the most remarkable. The colour of the gas within the larger parts of the tube through which Ruhmkorff's apparatus is discharged, is a dark blue, in the capillary part of the same tube the finest gold-colour, while the light surrounding the negative wire is of a fawn-colour [The finest appearance is obtained with a larger tube containing residual traces of the vapour of this salt, put on the iron-pieces of a powerful electro-magnet, within the blue light of the discharge numerous golden flashes are produced, and variously directed by the magnetic force]

A piece of metallic sodium within an atmosphere of rarefied hydrogen does not alter the spectrum of this gas when at the ordinary temperature, but when it is heated, a single brilliant yellow band is added to the three original bands of hydrogen The middle of the new band exactly coincides with Fraunhofer's dark line D The vapours of the metal are condensed in the cooler parts of the apparatus

Phosphorus, when treated in the same way, instead of adding new bands to those of hydrogen, at a certain temperature even extinguished the spectrum of the gas

In the case of mercury, I gave to the tube the shape of the annexed drawing The electrodes merely entered the two flask-shaped ends of the tube, where they were covered about half an inch with mercury Peculiar bands were obtained without heating the mercury, when it was heated, the spectrum became most brilliant.



When traces of two gases, not acting on each other, are mixed within a spectrum-tube, the spectra of both are simultaneously obtained

A result of some importance, following from the researches of which I have here given an abstract, is, that in all such dioptrical researches, where Fraunhofer's dark lines were used in order to get exact measures, these dark lines may with great advantage be replaced by the middle lines of the new brilliant bands of the gas spectra To these bands the most convenient breadth may be given in each particular case by regulating the aperture of the apparatus A spectrum-tube of hydrogen, exhibiting three well-defined bright bands, is well suited for this purpose. During a whole year I made use of such a tube, which remained absolutely unaltered

Every gas being characterized by its spectrum (even by one of the bands of the spectrum, the position of which is measured), we get a new kind of chemical analysis In this way only we may ascertain the residual contents of the exhausted tubes, and the changes they undergo, either suddenly by the discharge, or gradually afterwards Thus, for instance, a spectrum-tube in which traces of vapour of sulphuretted carbon were enclosed, presented most unexpectedly the combined spectra of hydrogen and oxide of carbon Hence we conclude that there remained also within the tube some traces of vapour of water, which was decomposed as well as the sulphuretted carbon Traces of sulphur were deposited on the interior surface of the tube, while oxide of carbon and hydrogen remained within it

Nearly all examined combinations of hydrogen with metals, with chlorine, &c, were almost instantly decomposed. The spectrum of hydrochloric acid was found to be the combined one of hydrogen and chlorine Sulphur and arsenic were deposited from their combinations with hydrogen, the former constituting fine dendrites, the latter well-defined large bands the spectrum in both cases was that of pure hydrogen Seleniated hydrogen showed within the capillary tube a fine yellow tint, but this tint was converted during the passage of the discharge into a brilliant red one, the change of colour started from one of the extremities of the capillary tube, and reached the other one after a few seconds The spectrum observed during this change of colour was in the first moment a most distinct one, showing, for instance, between Hβ and Hγ two similar systems of two brilliant blue bands separated by a black one about double as large, but this spectrum entirely disappeared, as in dissolving views, being by and by replaced by the spectrum of hydrogen A few minutes after the discharge was stopped the red colour turned again into the primitive yellow one, the gas decomposed by the discharge having been recomposed again Thus the same experiment could be repeated any number of times

[Similar chemical effects were ascertained in a different way in the case of sulphurous acid, which, if included within a larger tube, shows a fine stratification of narrow violet bands. When the discharge passes during several minutes, the stratification is altogether changed, the narrow violet bands being transformed into the large clouds of the best Torricellian vacuum tubes The primitive condition of the gas is restored by heating the electrodes, but in every new experiment the phenomena became less distinct than they were before.]

I think it most probable that, properly speaking, *electric light does not exist; the light which we see belongs to the gas, rendered incandescent by the thermal action of the current*. Accordingly, in our case, the colour of the appearing light depends upon the nature of the gas and the concentration of the current. This opinion is strongly supported by the observed fact, that the temperature of the capillary tube increases considerably in some instances. Considering that this increase of temperature has its source in the heat of the residual gas, which is too small in amount to be indicated by the balance, this heat being produced by the electric current, and communicated to the heavy substance of the tube; we have scarcely an idea of the *enormous* temperature of the gaseous electrode included in the capillary channel.*

II. "On the Interruption of the Voltaic Discharge in Vacuo by Magnetic Force." By J. P. Gassiot, Esq., F.R.S. Received December 6, 1859.

The late Professor Daniell, in his Fifth Letter on Voltaic Combinations (Phil. Trans. 1839, part 1), describes some experiments made with seventy series of his constant battery, and states (page 93) "that the arc of flame between the electrodes was found to be attracted and repelled by the poles of a magnet, according as one or the other pole was held over or below it, as was first ascertained by Sir H. Davy; and the repulsion was at times so great as to extinguish the flame."

In the Philosophical Magazine of July 1858, Mr. Grove has described an experiment made by him with one of my vacuum-tubes, 2 feet 9 inches long, in which he ascertained that the discharge of a Ruhmkorff's induction coil could be stopped by bringing a magnet near the positive terminal wire, but that this effect was not obtained when the magnet was made to approach the negative. The mercurial vacuum-tube in which Mr. Grove observed this phenomenon was unfortunately shortly afterwards broken, and although Mr Grove and myself have repeatedly endeavoured to obtain the same result in similar and in other vacuum-tubes (and since that period I have experimented with upwards of two hundred), all our efforts have been hitherto unsuccessful

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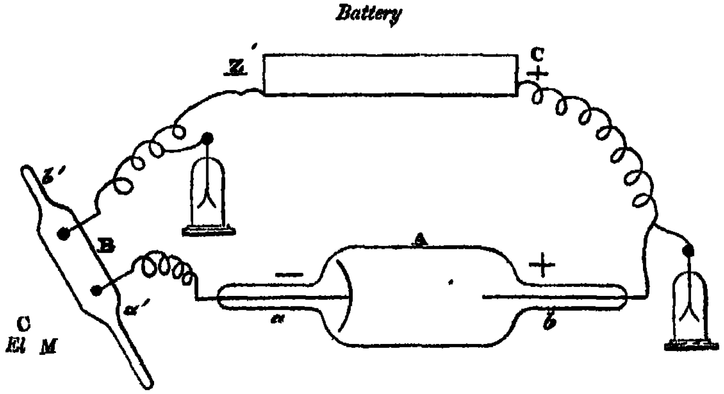
* In some peculiar cases my primitive theoretical views were modified, reformed, or extended by subsequent experiments. The abstract now given refers only to what I think *at present* to be the state of the question.

The experiments I am now about to describe were made with two carbonic acid vacuum-tubes, the vacua being obtained in the same manner as described by me in the Philosophical Transactions, part 1, 1859.

A, in the annexed figure, represents a glass tubular vessel (No 146), 24 inches long and 6 inches diameter in its wide part, at one end, attached to the platinum wire (a), is a concave copper plate 4 inches diameter, at the other end is a brass wire attached to the platinum wire (b) B represents a glass tube (196) 5 inches long (in its wide part), in which two small balls of gas-retort coke are attached to the platinum wires a' and b', and are placed about 3 inches apart, all the platinum wires being hermetically sealed in the glass. In A the potash is placed in the vessel between the electrodes, in B it is placed in the further part of the tube, beyond one of the wires

An electro-magnet is placed at C, and is constructed so as to allow the two helices to be separated, and by these means the larger vessel can, if required, be placed between them, and any portion of the luminous discharge may be thus exposed to any part of the magnetic field



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* The carbon balls do not in these experiments affect the results described, as I have obtained the same in a tube of the same dimension with brass wires.

When the terminals of an excited induction coil are attached to the wires of either of the above vacuum-tubes A or B, luminous discharges are obtained, the negative wire ball or plate being covered with a luminous cloud-like glow extending towards the positive, but the stratifications are not developed, except by the magnet, and these become more clearly defined as the magnet is caused to approach, or as the power is increased, when they are deflected according to the direction of the discharge, or of the polarity of the magnet. But with the induction coil, no matter how I reduced the intensity of the discharge, or varied that of the electro-magnet, in no instance could I produce in these, or in any of my vacuum-tubes, a similar result to that which Mr. Grove obtained in the vacuum-tube so unfortunately broken, the experiment evidently requires a certain balance of power between the electric discharge and that of the magnet, and this I had hitherto been unsuccessful in obtaining

I next experimented with my water-battery (Phil Trans 1844, and Proceedings, 26 May, 1859), which I have recently had carefully cleaned and recharged with rain-water, the luminous discharge in both the vacua A and B was obtained with less than 1000 series, and this discharge, as well as that from the full series of the battery of 3520 cells, was under certain conditions, hereafter described, entirely destroyed or interrupted by the power of the magnet

At first the interruption or break in the luminous discharge appeared to be caused by the sudden action of the magnet, as if it were merely momentarily blown out, for the discharge recovered itself while it remained under the influence of the magnet—the luminous discharge under this condition gradually reappearing stratified and strongly deflected, but I subsequently ascertained that, by carefully adjusting the intensity of the battery discharge, and the force or power of the electro-magnet, this recovery in the discharge could be entirely prevented.

On approaching the vacuum A towards the electro-magnet, the luminous discharge from the battery assumed the same form as that from the induction coil, but when the vacuum was placed between the helices, so as to permit the armatures or poles of the magnet to touch one or each side of the glass vessel at about its centre, the discharge disappeared, as soon as the magnet was removed, or the vacuum-tube withdrawn from its influence, the luminous discharge was reproduced.

To test whether a complete disruption of the electrical current had taken place, two gold-leaf electroscopes were attached, one to the zinc and the other to the copper terminal of the water-battery, the leaves diverged with considerable energy, connection was then made from the electroscopes to the wires of the vacuum-tube, the luminous discharge became visible, and the leaves of both electroscopes partially collapsed, the vacuum-tube was then placed as before, between the armatures of the electro-magnet, and immediately the magnet was excited, the luminous discharge disappeared, and the leaves of the electroscopes diverged to their original maximum extent, thus proving the disruption to be complete

If the smaller tube B is placed across both poles of the magnet, the luminous discharge at its centre assumes the appearance of being *nearly* separated into two parts, each part showing a tendency to rotate round the pole of the magnet on which it is placed, the one in an opposite direction to the other. I endeavoured to obtain a disruption of the battery discharge when in this state, and possibly with a more powerful electro-magnet this experiment would succeed, but although I reduced the intensity of the battery discharge and increased the power of my electro-magnet, I could not in this manner obtain an actual discontinuity of the battery discharge, but when the same vacuum-tube was placed in a longitudinal or equatorial position between the poles, or even approached them within three or four inches in that direction, an immediate interruption of the discharge took place

When both vacuum-tubes are placed in the battery circuit, the interruption can be shown in a very striking manner. the general arrangement of the apparatus represented in the figure shows how this experiment is made A is fixed on a wooden support. One wire (*b*) is attached to the copper terminal of the battery, the other wire (*a*) being connected to one of the wires in B, which is held by the hand, the other wire (*b'*) being connected with the zinc terminal of the battery, gold-leaf electroscopes being placed as before In this manner all the apparatus is fixed except B, which being held by the hand, and the connecting wires being flexible, can be placed in any required position

As long as the vacuums are at a sufficient distance from the action of the magnet, the luminous discharge is visible in both, and the leaves of the electroscopes partially collapse, but immediately the discharge in B is placed in the position described in the previous experiment, between the poles of the magnet, the discharges in *both* vacua instantly disappear, and the leaves of the electroscopes diverge to their original maximum

The actual position of what is termed the magnetical field, around and between the poles of a magnet, has been generally delineated by means of iron filings placed between the poles on a sheet of paper. Assuming the lines in which these particles arrange themselves to represent the direction of the power of the magnet, or the magnetic field, they also explain the actual position through which the vacuum-tube should be placed to obtain the preceding result, and in this manner to show by experiment that a voltaic discharge which has sufficient intensity to pass through a space of upwards of 6 inches in attenuated carbonic acid gas is not only interrupted, but absolutely and entirely arrested by magnetic force

Postscript (received Jan. 19, 1860) —In repeating the experiments with Dr. Tyndall in my laboratory, the disruption of the luminous discharge *in vacuo* from 400 cells of the nitric-acid battery was obtained · some most beautiful and striking results were obtained from the same battery on the 16th inst, on repeating the experiment in the Theatre of the Royal Institution, with its large electro-magnet, Dr. Faraday and Dr Tyndall being present

The large receiver (146) was placed between the poles of the electro-magnet, the lines of force going through it, electrodes equatorial. The stratified discharge was extinguished Subsequently, through the sinking of the battery, or some other cause, the stratifications disappeared, and the luminous glow which filled the entire tube remained. On now exciting the magnet with a battery of ten cells, effulgent strata were drawn out from the positive pole, and passing along the upper or under surface of the receiver, according to the direction of the current On making the circuit of the magnet, and breaking it immediately, the luminous strata rushed from the positive and then retreated, cloud following cloud with a deliberate motion, and appearing as if swallowed by the positive electrode

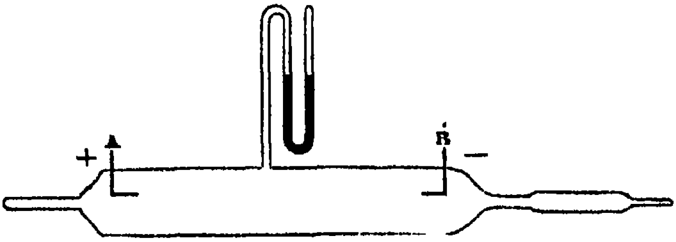
The amount of electricity which passed appeared materially increased on exciting the magnet, once the discharge was so intense as to fuse half an inch of the positive terminal

After this had occurred, the discharge no longer passed as before when the terminals of the battery were connected with it, but on connecting the positive end of the battery with the gas-pipes of the building, the discharge passed

The discharge could also be extinguished by the magnet, and the time necessary to accomplish this, furnished a beautiful indication of the gradual rise and reduction in the power of the electro-magnet

III. "On Vacua as indicated by the Mercurial Siphon-Gauge and the Electrical Discharge." By J P Gassiot, Esq., F R S Received January 19, 1860

That the varied condition of the stratified electrical discharge is due to the relative but always imperfect condition of the vacuum through which it is passed, is exemplified by the changes which take place in the form of the striæ while the potash is heated in a carbonic acid vacuum-tube. In order, if possible, to measure the pressure of the vapour, I had a carefully prepared siphon mercurial gauge sealed into a tube *fifteen* inches long, at an equal distance between the two wires A, B



This tube was charged with carbonic acid in the manner described by me in a former communication When exhausted by the air-pump and sealed, it showed a pressure indicated by about 0 5 inch difference in the level of the mercury, the potash was then heated, the mercury gradually fell, until it became perfectly level

Dr. Andrews (Phil Mag February 1852) has shown, that with a concentrated solution of caustic potassa, he obtained with carbonic acid a vacuum with the air-pump so perfect as to exercise no appreciable tension, as no difference in the level of the mercury in the siphon-gauge could be detected

On trying the discharge in the vacuum-tube after the potash had cooled, I found it gave the cloud-like stratifications, with a slight reddish tinge, consequently not only was the vacuum not perfect, as denoted by the form of stratification, but in this tube the colour denotes that even a trace of air remains,—probably that portion in the narrow part of the siphon-gauge, which, from its position, was not displaced by the carbonic acid

The potash was subsequently heated until the discharge was reduced to a wave-line, with very narrow striæ, in this state moisture is seen adhering to the sides of the tube, but even in this state the difference in the level of the mercury in the gauge did not ever vary more than 05 inch As the potash cooled, the discharge altered through all the well-known phases of the striæ, the mercury again becoming quite level.

At first almost the slightest heat applied to the potash alters the form of the stratifications, as the heating is repeated, longer application is necessary, but it shows how sensibly the electrical discharge denotes the perfection of a vacuum, which cannot be detected by the ordinary method of mercurial siphon-gauge

January 26, 1860.

Sir BENJAMIN C BRODIE, Bart., President, in the Chair.

The following communications were read —

I. "On the alteration of the Pitch of Sound by conduction through different Media." By SYDNEY RINGER, Esq., late Physicians' Assistant at University College Hospital. Communicated by Dr GARROD. Received November 25, 1859.

Having observed that the pitch of cardiac murmurs underwent various alterations dependent on the constitution of the conducting body, the following experiments were devised to extend and render more certain the observations made on the human subject. In most of these experiments a tuning-fork was used, and in all the alterations in pitch were tested by the ear.

In making these experiments, the note of the vibrating fork was first taken with the instrument close to the ear and without being in contact with any resounding body. It was next placed on the body which was experimented on, and lastly listened to through the medium of the same. The alteration in pitch obtained by these two latter methods gave always the same results in kind, but not in degree, the alteration being always greater when the note was heard through the medium of the conducting body.

SOLIDS

A board 13 feet long was balanced on the back of two chairs. The note of the fork was then taken, without its being in contact with the board. The fork in vibration being next placed at one end of the board, the ear was placed on the other, and the note was then found to be most appreciably lowered in pitch.

As boards composed of various kinds of wood were not obtainable, tables were used. Of all the woods thus tested, deal lowered the pitch most; indeed the lowering of the pitch was always in proportion to the porosity of the wood[†].

The pitch was found to fall the greater the distance from the fork.

*Bone lowered it.

Glass raised the pitch.

Iron raised it.

[†] Dr Wylde, the Conductor of the Philharmonic Society, kindly examined and fully corroborated, in those experiments marked with an asterisk, the conclusions I had previously come to. All the experiments were confirmed by numerous persons of acute ear. In no case was their opinion at variance with my own.

The two last, in conducting the note, greatly lessened the intensity, much more so than the substances described above.

The muscular substance of the heart lowered the pitch. Skin and cellular tissue, on the contrary, raised it

Liquids

A large foot-pan was filled with water, and the vibrating fork was partly introduced into this, but as no sound could be heard in this manner without some resounding body, a small circular piece of wood was used for this purpose, the fork placed on this was first listened to, the fork with the piece of wood was then placed under the water, one ear was then immersed in the water, and the note so taken, the pitch was then found to be most decidedly heightened Any objections to this method of performing the experiment were obviated by the following extension of it The eyes being firmly shut, any variation in the position of the fork, that is to say, whether it was moved closer to or further from the ear, was accurately determined by the alteration in the pitch.

Next, a glass tube 29 inches long, with a diameter of ¾ths of an inch, was closed at one end, with a diaphragm of gutta percha, oil-silk, or bladder (the same diaphragm being used in each set of comparative experiments) The tube was then filled with the fluid to be examined The ear being applied to the diaphragm, the stem of the vibrating fork was introduced above into the fluid, care being taken that neither the finger nor the fork was in contact with the glass. Experiments conducted in the above manner gave the following results

*Water raised the pitch most appreciably

*Alcohol still higher

Ether higher

A solution of protocarbonate of soda of the same specific gravity as the blood, raised the pitch more than pure water

* A saturated solution raised it still higher.

* Sulphate of baryta, suspended in water, raised it higher than any other tried fluid

Prussian blue, suspended in water, raised it more than water, but less than the sulphate of baryta

From the above results it appears that simple fluids heighten the pitch in proportion to their diminished specific gravity, and that the addition of any substance (though increasing the specific gravity), whether in solution, or merely suspended in the water, heightens it, that particles in suspension, indeed, heighten it more than solutions

The fact of different fluids raising the pitch in variable degrees, excludes the possibility of the rise being due to the glass, or any other material used, unless the fluid varying in weight altered the pitch, by affecting the tension of the diaphragm, but the fact of the alteration in pitch bearing no relation to the specific gravity of the fluid excludes this source of error.

The following experiments were devised to test the influence of running water on the pitch

Into an india-rubber tube, 13 inches long, and ¾ths of an inch diameter, a funnel was inserted, immediately below this a small opening was made, just large enough to admit the end of the fork Water was kept constantly running through this, and the stethoscope (covered with a diaphragm) applied to different parts of the tube, by this method the pitch was found to be most appreciably raised the further from the fork the stethoscope was applied to the tube The elevation of pitch was easily recognized at a distance of 2½ inches (the length of the pulmonary artery and adjoining part of the aorta)

The stethoscope having been unfortunately left behind, Dr Wylde could only apply the ear directly to the tube, and therefore could not speak so decidedly as he did concerning the other experiments, but he was of opinion that the pitch was raised as stated above.

It was next attempted to be ascertained whether the mere motion of the water increased or diminished the rise of the pitch It appeared that the pitch was very slightly raised by the mere motion of the fluid, the same point of the tube being listened to. The difference in *intensity* was most marked

The chief object of these experiments being to ascertain the influence of the different constituents of the human body on the pitch of cardiac and other murmurs, and in order that the experiments might, as closely as possible, simulate the actual phenomena in the body, an aorta was tied to the mouth of a tap, and an artificial murmur produced by causing a constriction of the vessel by a piece of twine tied round it. The pitch of the murmur so produced was decidedly raised the further it was heard along the vessel from the point where the sound was generated.

To set the question quite at rest of the possibility of the blood in a vessel raising the pitch, especially at so short a distance as 2½ inches, the following experiment was devised —A tourniquet was placed over a man's femoral artery, immediately below Poupart's ligament, and an artificial murmur thus produced, this was found to rise rapidly in pitch in passing down the course of the vessel. A well-marked difference was noticed at a distance of only an inch, and decidedly more at a distance of 2½ inches

The *intensity* of the murmur quickly diminished in passing to the right or left of the vessel, the *pitch* being at the same time rapidly raised, which was due to the interposition of integuments, but this interposition could not be the cause of the rise of the pitch in the course of the vessel, as the murmur could be heard in that direction at a distance of at least 6 inches, whilst it was completely lost at less distance than 2 inches to either side of the vessel, thus the murmur must have been conducted by the blood, whilst the same thickness of integuments was over the artery at the lower and the upper point listened to, for both points were above the place where the sartorius muscle crosses the vessel

GASES

If a watch is pressed close to the ear and then gradually moved away, the tick is heard to rise in pitch in proportion to the distance the watch is withdrawn

*Or, if in place of the watch a tuning-fork be used, the same can be still more distinctly ascertained. Then let the fork, either freely vibrating, or, still better, placed on a resounding board, be moved gradually away from the ear, the pitch will be found to rise the further the fork is carried away from the ear

An echo of a musical note is higher pitched than the original note. Again, a loud cardiac murmur audible over the entire chest was examined in the following manner·—

The patient was directed first to exspire to the utmost, and the pitch of the heart-sound was then ascertained, he was then ordered to inspire to the full, the pitch was then found to be raised. In this experiment, the only variation was an increased amount of air between the point where the murmur was generated, and the ear of the observer.

The substances which lowered the pitch in the above experiments have one common property, namely, porosity, and, as far it could be ascertained, the depression of pitch was in proportion to this condition. Is it possible that the small vacuities included in the substance, acting as resounding cavities, and reflecting the vibrations from their walls, may so direct them that they may somewhat interfere with one another, and thereby be somewhat diminished in number? The following experiment tends in some degree to support this conjecture It is well known that if the vibrating fork be held obliquely, resting on the table, "a loud resonance is audible, but if the tuning-fork be moved parallel to itself along the surface of the table, the resonance of the table immediately ceases from the interference of the planes of vibration with each other," but if the fork is moved so slowly, and so that the resonance is not completely destroyed, the pitch falls slightly

Again, if the fork be applied to the head, and listened to first with the ear open, and afterwards with the ear closed, the pitch is found to be slightly lowered

In all those experiments in which the pitch was elevated by conduction, it was found that there was diminished *intensity* in proportion to the elevation of *pitch*, thus it would appear that all bodies raise the pitch in proportion to the difficulty with which they receive and conduct vibrations

Dr Scott Alison has proved in some recent experiments, that the conductivity of media, as regards rapidity, does not correspond with that of intensity. Of all tried substances, iron was the worst conductor as regards intensity, and this was found to raise the pitch most

The above explanation is rendered somewhat probable from the fact that in all cases the elevation was greater with a weak note than a strong one Dr Wylde tells me that it has long been noticed by musicians that a weak note is somewhat higher pitched than a strong one, it being under these circumstances caught through the medium of the air Those bodies which, on the contrary, lower the pitch, do so to a greater extent with a weak note.

These explanations are offered with the utmost diffidence, on account of my very limited knowledge of acoustics.

On looking into the literature of the subject, the only reference to alteration of pitch by conduction is that by Dr Walshe*, who ascribes it to transmission of the vibrations through "varying" media.

II "On the frequent occurrence of Phosphate of Lime, in the crystalline form, in Human Urine, and on its pathological importance" By ARTHUR HILL HASSALL, M D Lond Communicated by Dr SHARPEY, Sec R.S Received November 9, 1859

In 1854 I submitted to the Royal Society a paper "On the frequent occurrence of Indigo in Human Urine." This communication, which was published in the 'Philosophical Transactions,' attracted considerable attention both at home and abroad The singular fact of the frequent presence of indigo in the urine, first announced by me, has since been amply confirmed by a variety of observers I have now to place before the Society some investigations in relation to the not uncommon occurrence in human urine of *phosphate of lime*, as a *deposit*, in a well-marked *crystalline* form

When the earthy phosphates are treated of by writers, in connexion with the urine, they are usually described collectively, and it is seldom that each kind of phosphate is particularized, and yet there are several which may occur either separately or together. The phosphate of ammonia and magnesia, or triple phosphate, is indeed often specified, but rarely is phosphate of lime separately mentioned, and phosphate of magnesia scarcely ever, and yet phosphate of lime is very frequently present as a deposit in urine, much more so, indeed, according to my experience, than the triple phosphate, excluding those cases of the occurrence of that ammoniacal phosphate, arising from the decomposition of the urea of the urine subsequent to its escape from the kidneys Even in those few cases in which phosphate of lime is specially mentioned, it is described *usually* as mixed up with the other phosphates, and *always* as occurring in the *amorphous* or *granular*, and never in the crystalline state, further, no peculiar importance is attached to it, as contrasted with the magnesian phosphate

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* Disease of the Lungs, Heart, and Aorta 2nd edition, page 151.

Even one of the most recent writers on the urine gives the following description of the physical characters of deposits of phosphate of lime in urine:—"Deposits of phosphate of lime," he states, "as usually occurring in the urine, and mixed with magnesia, are always white and amorphous, under the microscope appearing in granules, sometimes of a greenish tinge, which exert a refracting action upon light. *Crystallized deposits of this substance have not been observed.*"

I now propose to show, *first,* that there is a crystalline deposit, the crystals composing which will be described hereafter, which does really consist of phosphate of lime; *second,* that it is of frequent occurrence in human urine; and *third,* that it is of greater pathological importance than the deposits of triple phosphate.

I would first remark that I have for years been acquainted with the fact of the occurrence of crystalline phosphate of lime in the urine, and I have referred to it in 'The Lancet' of 1853, and also elsewhere. I should now remark, however, that the statement made by me as to the composition of the crystals, has hitherto been based upon their *qualitative* analysis only, and therefore was not so completely conclusive and satisfactory as could be desired. Until recently I had not made any *quantitative* analyses; these I have since been enabled to perform, and I now furnish the results of the chemical examination of four samples of the deposit.

First Sample.—Filtered from the urine of twenty-four hours; mixed, as ascertained in the first instance by means of the microscope, with a very minute quantity of *triple phosphate.*

Bibasic phosphate of magnesia ..	0·15
Bibasic phosphate of lime ..	1·85
	2·00

Second Sample.—Filtered from urine after the lapse of a day or two; mixed with a small quantity of *triple phosphate.*

Bibasic phosphate of magnesia ..	0·47
Bibasic phosphate of lime.... ..	6·18
	6·65

Third Sample.—From urine of twenty-four hours, after the lapse of several days. Admixed with much triple phosphate, as shown first by the microscope, and afterwards by the chemical analysis

Bibasic phosphate of magnesia	4 30
Bibasic phosphate of lime .	5 41
	9 71

Fourth Sample.—Separated from six ounces of fresh urine Deposit very pure.

Bibasic phosphate of lime . .	1 96

No phosphate of magnesia.

Now the admixture of the phosphate of magnesia in the first three samples was due solely to the fact that the phosphate of lime, deposited at first in the pure state, was allowed to remain in the urine until decomposition had commenced, and the phosphate of magnesia and ammonia had, in consequence, become formed. Deposits of phosphate of lime are sometimes contaminated from the same cause with carbonate and oxalate of lime

These analyses are therefore conclusive as to the composition of this earthy phosphate In order to show that no error has been committed in them, I here append the process adopted. That the deposit in question really consisted of a *phosphate*, was first repeatedly determined by the action of a solution of nitrate of silver, the crystals when touched with this reagent assumed a bright golden yellow colour. After having been separated and washed in distilled water, the phosphate was ignited to free it from animal matter, urea, &c, and weighed It was then dissolved in hydrochloric acid, ammonia was added until a permanent precipitate formed, this was redissolved by the addition of acetic acid First the *lime* was precipitated from the solution by oxalate of ammonia, and afterwards the *magnesia* as follows. chloride of ammonium was added, then ammonia in slight excess, and lastly, phosphate of soda The oxalate of lime formed was converted into carbonate of lime in the ordinary manner, and the phosphate of ammonia and magnesia into the pyro-phosphate of magnesia, these were then weighed separately, and the amounts of the bibasic phosphate of lime were determined by the usual calculations. The results obtained corresponded very closely with the original weights of the ignited phosphates subjected to analysis The analyses, therefore, show that the crystallized phosphate of lime is a tribasic phosphate containing two atoms of lime, and most probably one of water

Form of the Crystals.

The size, form, and arrangement of the crystals of phosphate of lime, as they occur in human urine, vary greatly, but the peculiarities are in all cases sufficiently characteristic to allow of the ready identification of this phosphate by means of the microscope. The crystals are either single or aggregated, most frequently the latter, forming glomeruli or rosettes, more or less perfect (figs. 1, 2, 3) Sometimes they are small and needle-like, and then they frequently form by their crossing and union at right angles, glomeruli or spherules (fig. 3) Sometimes the crystals are thin and flat, having oblique or pointed terminations (fig. 2). Very frequently, however, they are thick, and more or less wedge-shaped, and united by their narrow extremities so as to form more or less complete portions of a circle (fig. 1); the free larger ends of the crystals are usually somewhat oblique, and the more perfect crystals present a six-sided facette. I have never yet met with these crystals having both ends perfect, owing, I believe, to the tendency which they have to crystallize from a centre

Fig 1

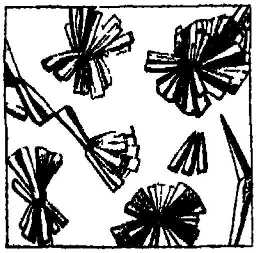


Fig 2.

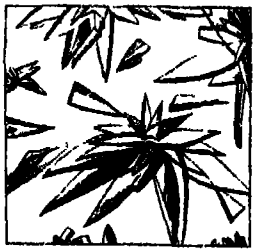


Fig 3

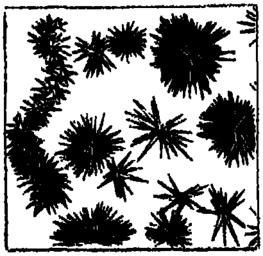
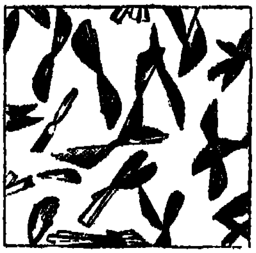


Fig 4



in rosettes The principal modifications which I have met with in the size, form, and arrangement of these crystals, are well shown in the eleven drawings which accompany this communication*. When these crystals are kept in the dry state for a long time, they not unfrequently break down and crumble into powder

The late Dr Golding Bird, in his work on 'Urinary Deposits,' has given a representation of some crystals which he has denominated "*penniform,*" describing them as consisting of a variety of the magnesian phosphate, the crystals figured do, however, undoubtedly represent a modification of those of *phosphate of lime.* They are represented in figure 4, taken from Dr. Bird's original specimen Although I have elsewhere pointed out this error, most recent writers on the urine still persist in describing these crystals as a variety of the phosphate of ammonia and magnesia

On the Frequency of their Occurrence

I find, as already stated, that phosphate of lime in the form of crystals is of much more frequent occurrence in human urine than the triple phosphate, excluding those cases of the presence of the latter phosphate which are due to the decomposition of the urea of the urine subsequent to its emission I have met with deposits of crystallized phosphate of lime in some hundreds of urines, and in many different cases, it is therefore not a little remarkable, from the frequency of its occurrence and the peculiarities presented by the crystals, that it should have been so long overlooked The microscope therefore furnishes us in most cases with the ready means of detecting the presence of deposits of phosphate of lime, as of so many other urinary deposits

Characters of Urine depositing Crystallized Phosphate of Lime

The urine from which phosphate of lime is deposited is usually pale, but occasionally it is high-coloured; the quantity passed is large, and the calls to void it frequent, more or less uneasiness and smarting being occasioned by its passage, at the neck of the bladder and along the course of the urethra: its specific gravity varies greatly; taking the whole quantity passed in twenty-four hours, it is usually below the average, nevertheless the animal matter and urea are absolutely in excess It is generally feebly acid, often decidedly so when first voided, the greater part of the phosphate of lime becoming deposited while the urine still retains some degree of acidity, it however speedily becomes alkaline, owing probably to the excess of mucus contained in it Sometimes the crystals of phosphate of lime are thrown down from the urine before its escape from the bladder, ordinarily, however, the urine is bright and clear when passed, and the crystals are not formed until some time after it has been voided In collecting this phosphate for analysis, the object being to procure it in as pure a state as possible and as free from phosphate of ammonia and magnesia, oxalate and carbonate of lime, it should be separated from the urine very soon after it has become deposited, and before decomposition has had time to set in

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* The Figures on the preceding page represent selected portions of four of these drawings all the objects are magnified 100 diameters

On the Pathological Importance of Deposits of Phosphate of Lime in Human Urine

Of the pathological importance of excess of phosphate of lime in the urine not a doubt can be entertained, but certain reasons and facts may be advanced to show that deposits of that phosphate have a deeper pathological significance than those of the phosphate of ammonia and magnesia. The proof of this is the more necessary, since writers on the urine are in the habit of describing, as well as of treating, deposits of the earthy phosphates collectively, and without distinguishing between them · this course was natural enough so long as they were unacquainted with the fact that deposits of phosphate of lime in the state of crystals are of frequent occurrence, or so long as they mistook them for a variety of the ammonio-magnesian phosphate. One reason why we should be disposed to attach greater importance to the excess of the calcareous than the magnesian phosphate, is that most of the phosphoric acid of this last phosphate, and all the magnesia, is derived from *without*, being contained in the various articles consumed as food, while for the phosphate of lime, we have in the system—in the teeth and bones, and also in the nitrogenous tissues—sources containing some pounds weight of this phosphate.

That the osseous system is subject to disintegration is certain, and that the extent and rapidity of this differ remarkably in different cases is equally so. This is shown by the simple fact alone of the early and rapid decay of the teeth in many persons For this general reason therefore only, we should, *a priori*, be disposed to attach greater importance to the occurrence of deposits of phosphate of lime than those of phosphate of magnesia

Other facts tending to confirm this view are, first, that while deposits of phosphate of lime are frequently met with, those of phosphate of magnesia (not the ammonio-magnesian phosphate) are exceedingly rare, and second, that the calcareous is of more difficult solubility than the magnesian phosphate. This last circumstance explains probably why phosphate of lime falls as a deposit from acid urine, while phosphate of magnesia remains in solution

The particular or special reasons for regarding deposits of phosphate of lime as of more moment than those of the triple phosphate, are derived from direct pathological observation. I have observed that when this deposit occurs, it is very apt to be persistent, and when it has disappeared, to return whenever the health is reduced from any cause. I have also noticed that, when it is persistent, it is usually associated with marked impairment of the health, and this often where organic disease does not exist. The prominent symptoms in one case of calcareous phosphatic deposit which I have had under observation for some years, were,—great disorder of the digestive organs, frequent and distressing headaches, occasional vomiting, debility, emaciation, great irritability of the nervous system, sexual powers weak, pulse slow and feeble, skin cold, urine in excess, of rather low specific gravity, acid when passed, but soon becoming alkaline, micturition frequent, with irritation at neck of bladder and in the course of the urethra · teeth much decayed It should be stated that there is in this case a very slight tendency to paralysis of the right leg, as shown by an occasional sensation of coldness in the limb, and slight deficiency of power in it at times only This symptom is, however, by no means a constant or necessary one in such cases

If these views of the pathology of phosphate of lime be correct, we should expect to find an excess of that phosphate in the urine in great and rapid waste of tissue, during the rapid decay of the teeth, and in cases of *mollities ossium* That there is an excess of the calcareous phosphate in the urine in these cases, is shown alike by observation and analysis.

It is obvious from this imperfect sketch that much remains to be effected in regard to the pathology of phosphate of lime, but now that the frequency of its occurrence in human urine as a crystallized deposit is made known, its pathology, apart from that of the triple phosphate, will no doubt be specially considered.

It will be apparent from the following quotation, that the late Dr Golding Bird regarded deposits of phosphate of lime as of more consequence than those of the triple phosphate —"The pathological state of the system accompanying the appearance of deposits of phosphate of lime is analogous to that occurring with the triple phosphate, indeed, as has been already observed, they often, and in alkaline urine always, occur simultaneously So far as my own experience has extended, when the deposit has consisted chiefly of the calcareous salt, the patients have appeared to present more marked evidence of exhaustion, and of the previous existence of some drain on the nervous system, than when the triple salt alone existed, unless its source is strictly local"

It should be remembered that these remarks of Dr Bird refer to deposits of phosphate of lime in *the granular state*, and not to the crystalline deposits, with the occurrence of which he was unacquainted I have already stated that, according to my experience, the granular calcareous phosphatic deposits are much more rare than the crystalline.

It follows from these observations and investigations —

First. That deposits of *crystallised* phosphate of lime are of frequent occurrence in human urine, much more so, indeed, than those of the amorphous or granular form of that phosphate

Second That the crystals present well-marked and highly characteristic forms, whereby the identification of this phosphate by means of the microscope is rendered easy and certain

Third. That there is good reason to believe that deposits of phosphate of lime are of greater pathological importance than those of the phosphate of ammonia and magnesia.

February 2, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with notice given at the last meeting, the Right Honourable Sir Edward Ryan, Member of Her Majesty's Privy Council, was proposed for election and immediate ballot, and the ballot having been taken, Sir Edward Ryan was declared duly elected

The following communications were read:—

I "On the Saccharine Function of the Liver" By George Harley, M D, F C.S., Professor of Medical Jurisprudence in University College, London Communicated by Dr. Sharpey, Sec R S. Received December 19, 1859

Although it is nearly 200 years since our countryman, the celebrated Dr Thomas Willis, made the important discovery of the *occasional presence of sugar in the human urine*, it was not, until very recently, known that the formation of saccharine matter is constantly going on in the healthy animal body.

Since Bernard, in 1848, communicated to the French Academy the discovery of the saccharine function of the animal organism, physiologists in all countries have more or less directed to it their attention. For a time various opinions were held by different observers regarding the origin of the sugar found in the body, but at length it was generally admitted that the liver had the power of forming a substance to which Bernard gave the name *Glucogen*, that this peculiar substance was transformed into sugar, and that the sugar in its turn disappeared in the capillaries of the different organs and tissues of the body.

In the summer of 1858, however, Dr. Pavy read a paper on the "Alleged Sugar-forming Function of the Liver" before the Royal Society, the object of which was to prove that the presence of sugar in the animal economy is "due to a *post mortem* occurrence,"—that as long as life continues, glucogen only is to be found, and not until after death does the transformation of this substance into sugar begin.

The question of the saccharine function of the liver being a subject to which I have more or less directed my attention since 1853, when I communicated to the Société de Biologie de Paris an account of an experimental procedure whereby diabetes can be produced artificially in animals, the above-mentioned paper was to me one of peculiar interest. The conclusions of the author were so much opposed to the results of my own experiments, as well as those of other observers, that I felt anxious to test them.

Accordingly, having received the kind offer of Professor Sharpey's cooperation, I undertook a series of experiments, the results of which I beg the honour of communicating to the Royal Society

As the experiments performed were merely a repetition of some of those made by previous inquirers, I shall not enter into detail further than is necessary to explain the precautions adopted with the view to avoid error And, looking at the object in view, it will readily be understood why in the present instance the tests employed for the detection of the sugar were limited to caustic potash with and without sulphate of copper The mode of proceeding was as follows ·—In testing the blood, a quantity of distilled water, equal to about four times that of the blood used, was boiled in a capsule. To the water, when boiling, were added a few drops of acetic acid, and afterwards the blood was very gradually introduced In order that the albumen might be thoroughly coagulated, a drop or two more of acetic acid was added, care being taken to avoid an excess When the albumen was completely coagulated, which was known by its separating and floating in the clear liquid, the whole was thrown on a filter. The clear filtered liquid was then tested The same process was followed when operating on the liver

The first point to be ascertained was whether, under favourable circumstances as regarded diet, sugar could be found in the circulation The following experiment proved this

Exp. 1. From the carotid artery of a rough terrier dog, three hours after being fed on bread, milk, and boiled liver, a portion of blood equal to about three-fourths of an ounce was withdrawn This, on being treated in the manner explained, gave distinct evidence of the presence of sugar. A second portion of blood, after standing thirty-five minutes in a room of moderate temperature, yielded a similar result

As in this instance a few seconds elapsed between the withdrawal of the first portion of blood and its treatment with the boiling acidulated water, and as it was possible that in these few seconds the sugar might have been formed from the glucogen present in the circulation, we (Professor Sharpey and myself) thought it advisable in our next experiment to allow the blood to flow directly from the artery into the boiling mixture, and thereby avoid the possibility of sugar being produced by the transformation of glucogen after the removal of the blood from the body. It was further desired to operate on an animal in what might be considered its natural condition as to food Accordingly one that had been running at large was selected, and the following experiment performed :—

Exp 2 Into the left carotid artery of a small cocker dog was inserted a canula with a stopcock The animal was then placed so as to allow the blood to flow directly into the boiling acidulated water The clear filtered liquid from this blood became of a yellow tint on being boiled with soda, and gave a red precipitate with the sulphate of copper and potash, thereby indicating the presence of sugar. Two ounces of blood from the same animal were similarly tested after the blood had stood twenty-four hours in a room of moderate temperature, and the result obtained was the same as with the first portion

The next experiment was made on an animal under conditions, as regards food, unfavourable for the production of sugar. In order, too, to avoid any chance of injuring the sympathetic nerve during the operation, and thereby favouring the formation of sugar in the body, the blood was withdrawn from the right femoral artery instead of the carotid. The following are the particulars of the experiment :—

Exp. 3. A good-sized dog was fed solely on flesh during four days. Three hours after the last meal, which consisted of half a pound of boiled horseflesh, 1½ oz of blood was permitted to flow from the femoral artery directly into the boiling mixture. The solution obtained from this blood, as in the other cases, contained sugar. Another portion of blood, after standing three hours, was tested in the same way, and, as far as could be judged by the eye, contained a similar proportion of sugar.

In neither of the preceding cases was the amount of sugar in the blood quantitatively determined, as I had already done so on many previous occasions; and I knew that in healthy arterial blood it varied according to the state of the digestion, and the kind of food, from an inappreciable quantity up to 0·24 per cent *

Having been now satisfied that sugar is to be found in the blood of healthy animals at the very moment of its withdrawal from the circulation, even when none has been introduced along with the food, we next proceeded to test the grounds upon which it had been asserted that glucogen is not transformed into sugar in the healthy liver during life.

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* "On the Physiology of Saccharine Urine," by Geo. Harley, M D, British and Foreign Med. Chir. Rev July 1857, p. 191–204

In the paper already referred to, Dr. Pavy stated that the sudden abstraction of heat from the liver after its removal from the body, checks the transformation of the sugar-forming material, and thereby enables us to operate on the hepatic substance while in the same chemical condition as during life. The plan he recommends is to sacrifice a dog by pithing, and instantly to slice off a piece of liver, and throw it into a freezing mixture of ice and salt In which case he says the absence of sugar is almost complete, and thence concludes that the presence of sugar in the liver can no longer be looked upon as a "natural *ante mortem* condition," but "is in reality due to a *post mortem* occurrence"

In the following experiments, not only was the plan recommended most scrupulously followed, but even the risk of the glucogen in the liver becoming transformed into sugar during the process of preparing the decoction, was avoided by cutting the frozen liver into thin slices, and allowing them, while still in that condition, to fall directly into the boiling mixture of acetic acid and water The liver was in this way prevented from thawing until it entered a medium as capable of arresting the transformation of its glucogen into sugar as the cold. The decoction so obtained might therefore be presumed to contain the soluble matters as nearly as possible in the same chemical state as they were in the living organ.

Exp. 4. A small, but full-grown dog was fed during fourteen days solely on animal food. Four hours after a meal of boiled horseflesh he was killed by section of the medulla oblongata. The abdomen was rapidly opened, and a portion of liver cut off and instantly immersed in a freezing mixture of ice and salt. A second portion of liver was as speedily as possible detached, and quickly washed in cold water. The latter portion was then, without loss of time, cut into fragments, which were allowed to fall directly into boiling acidulated water. On testing the clear filtrate, distinct evidence of the presence of sugar was obtained. After half an hour, the frozen portion of liver was taken, without being allowed to thaw, and sliced directly into the boiling water with acetic acid. The clear liquid yielded in this case as distinct evidence of sugar as in the other Forty minutes after the death of the animal, another portion of liver, which till then had remained undisturbed in the abdomen, was treated like the others This gave evidence of containing a much greater quantity of sugar, thus confirming Bernard's statement, that the transformation of glucogen goes on in the liver after its removal from the body, or after the death of the animal

In order to be perfectly certain that the sugar found in the liver at the instant of its removal from the body was really formed where it was found, and not carried there by the portal blood from the food, the following experiment was performed.—

Exp 5. A dog was fed during ten days on boiled tripe Twenty-two hours after the last meal the animal was pithed In less than twenty seconds a portion of the liver was in the freezing mixture of ice and salt While I boiled directly another portion of liver, Professor Sharpey put a ligature on the portal vein, and collected its blood He likewise collected some of the hepatic blood which flowed from the cut liver.

In the portal blood not a trace of sugar could be detected The hepatic blood, on the other hand, gave distinct evidence of its presence Both bloods were tested exactly alike The clear liquids obtained from the frozen liver and from the portion treated directly, notwithstanding that they were filtered while hot, and also tested while still hot, both gave distinct evidence of sugar. On the following day a second portion of portal blood, which had been purposely kept all night in order to ascertain if, on standing some time, sugar would form in it, still yielded the same negative result. Even after treating it with saliva, which would have transformed its glucogen into sugar, had it contained any, no evidence of the presence of sugar was obtained. On the other hand, when saliva was added to the decoctions of the liver above spoken of, a great increase in the amount of sugar was observed. The quantity of sugar so obtained did not appear to be so great, however, as that yielded by a portion of the liver which remained all night untouched in the abdomen of the animal.

Professor Garrod, F.R.S., who was present, not at the commencement of the experiment, but on the following day, when the different decoctions were tested, agreed with Professor Sharpey and myself, that this experiment showed the truth of Bernard's statement, that the liver might contain both sugar and glucogen when the portal blood contained neither

The stomach and intestines of this animal were found void of food, the large intestine only contained fæcal matter.

For the sake of still further assurance that the sugar found in the liver was neither due to some accidental cause, nor immediately derived from food, we determined to deprive an animal of food for some days before examining the liver The following experiment was accordingly performed.—

Exp. 6 A very large and powerful dog, in admirable condition, was subject to a rigid fast for seventy-two hours—three full days Immediately after death, by section of the medulla oblongata, a portion of the liver was sliced off and immersed in ice and salt. Blood was then collected from the following sources —

1st From the portal vein.

2ndly. From the liver (*i.e.* blood which flowed from the liver when a portion of it was sliced off)

3rdly. From the right side of the heart

4thly. From the aorta

5thly. From the inferior vena cava.

Although these bloods were all treated in a similar manner, and tested with the same quantities of copper and soda, yet none of them gave unequivocal evidence of the presence of sugar, except that from the liver. The blood from the right side of the heart gave doubtful evidence. At first sight it may appear strange that the blood from the right side of the heart should contain scarcely an appreciable quantity of sugar, while that of the liver showed its presence very obviously, but this no doubt arose from the hepatic blood being in great part prevented from reaching the heart: 1st, on account of most of it escaping into the abdomen, when the portion of liver was cut off; and 2ndly, on account of its flow being in great measure arrested by the ligature of the portal vessels.

All the bloods, except the hepatic, seemed to be free of glucogen as well as sugar, for none of them, with that exception, gave any evidence of its presence after being treated with saliva in the usual way

On examination of the frozen liver (after three hours), which, as in the other cases, was not allowed to thaw before being put into boiling water, the decoction was found to reduce the copper readily

On opening the stomach, nothing was found in it except some neutral mucus The intestines were equally destitute of food, and in the rectum only a very small quantity of fæces was found, so there could be no doubt as to the animal being in a fasting condition

The only point now remaining, was to determine quantitatively the increase in the amount of sugar in the liver after its removal from the body, and for that purpose we preferred operating on an animal fed on a mixed diet

Exp 7 A small dog, which had been previously fed on animal diet, received a full meal of bread and milk Five hours afterwards the animal was pithed, and a portion of the liver rapidly sliced off and immersed in a freezing mixture A ligature was placed on the portal vein, and its blood collected before the circulation had ceased.

On examination, this blood was found to contain a small quantity of sugar, derived no doubt from the food. Bernard, I believe, has erred in supposing that all the saccharine matter found in the animal organism is formed out of the glucogen produced in the liver. This, no doubt, is the case in the carnivora when the diet is restricted to food inconvertible into sugar in the alimentary canal, but cannot be regarded as the natural state of things either in the omnivora or herbivora; for the food of the latter not only contains sugar, but its amylaceous elements may be converted into that substance in the process of digestion. The sugar found in the bodies of animals fed on a mixed diet ought therefore to be regarded partly as the direct product of the food, and partly as derived from the glucogen formed in the liver.

Bernard's chief argument against this view is founded on the fact that the livers of dogs fed on a mixed diet contain no more sugar than those fed on purely animal food In my opinion, however, this fact is not sufficient to decide the question; for, as the liver does not store up sugar, the quantity it at any time contains is no criterion of the amount produced in it. Moreover, the sugar derived from the food need not be expected to be found in the liver. Had Bernard gauged the sugar present in the blood, instead of that in the liver, after each kind of diet, the result obtained would, I believe, have led him to a different conclusion This being a point of great practical importance in the treatment of diabetes, I may be here permitted to mention that I have occasionally found nearly twice as much sugar in the blood of an animal on a mixed, than in that of one feeding on a purely flesh diet.

To return to the last experiment About two hours after the death of the animal, portions of the frozen part of the liver, and of that which had been kept warm in the body of the animal, were carefully weighed, and the proportions of sugar they respectively contained estimated by volumetric analysis.

The portion of frozen liver was found to contain 0 333 per cent, and that of the other 1·55 per cent. of saccharine matter It is thus seen that in two hours the sugar in the liver had augmented nearly fivefold. As Bernard has shown, the simple washing out of the liver by passing a stream of water through its vessels, would remove all the sugar anteriorly formed On placing it again aside for a short time, a fresh portion of sugar would form in it at the expense of glucogen

0·333 per cent of sugar seems a small quantity, but if we suppose a liver weighing, as in man, not less than 50 oz., to contain 0 333 per cent., above 70 grs. of sugar would be present in it at the moment of death,—no very insignificant quantity, when it is recollected that sugar is removed from the liver with every pulsation of the heart, to be partly consumed, and that it is as continually supplied by the organ.

The results of the experiments now related do not therefore in any way countenance the notion that sugar is not produced in the healthy animal body On the contrary, such conclusions as they afford are altogether in favour of the generally received views upon the subject.

From the preceding experiments the following conclusions may be drawn:—

1st. Sugar is a normal constituent of the blood of the general circulation.

2ndly. Portal blood of an animal on mixed diet contains sugar.

3rdly. Portal blood of a fasting animal, as well as of an animal fed solely on flesh, is devoid of sugar.

4thly The livers of dogs contain sugar, whether the diet is animal or vegetable

5thly. Under favourable circumstances, saccharine matter may be found in the liver of an animal after three entire days of rigid fasting.

6thly The sugar found in the bodies of animals fed on mixed food is partly derived directly from the food, partly formed in the liver.

7thly The livers of animals restricted to flesh diet possess the power of forming glucogen, which glucogen is at least in part transformed into sugar in the liver,—an inference which does not exclude the probability of glucogen (like starch in the vegetable organism) being transformed into other materials besides sugar

8thly As sugar is found in the liver at the moment of death, its presence cannot properly be ascribed to a *post mortem* change, but is to be regarded as the result of a natural condition.

II "Hereditary Transmission of an Epileptiform Affection accidentally produced." By E Brown-Séquard, M.D. Communicated by Dr Sharpey, Sec. R.S. Received December 23, 1859.

It is well known that the number of facts which seem to prove that an accidentally produced affection may be transmitted by parents to their offspring is still small, and that serious objections have been raised against most, if not all, the facts of this kind The following observations seem to show peremptorily that, at least in one species of animals, this kind of transmission may occur.

I have shown that certain injuries to the spinal cord, in Guinea-pigs and other Mammals, are followed, after a few weeks, by a convulsive disease, very much like epilepsy. For several years it has been frequently observed that the young of a number of those epileptic animals, which I kept in my laboratory, were at times attacked with epileptiform convulsions. For many months I have made regular observations on this curious subject, and I have ascertained, by careful watching, that six young guinea-pigs which had frequent attacks of convulsions, were the offspring of one male and two female guinea-pigs rendered epileptic in consequence of an injury to the spinal cord.

This observation derives its importance chiefly from the fact that, if epilepsy is an affection which naturally exists among guinea-pigs, it must be very rare, as I have never seen it except in the progeny of individuals operated upon and rendered epileptic, and yet the number of healthy guinea-pigs that I have kept for months is really immense It seems therefore that we can conclude, from these observations, that epilepsy, or an affection which very much resembles it, may be transmitted from parents to offspring, even when it has been accidentally produced in one of the parents,—at least in one species of animals

February 9, 1860

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair

The Right Honourable Sir Edward Ryan was admitted into the Society.

The following communications were read:—

I. "On the Resin of the *Ficus rubiginosa*, and a new Homologue of Benzylic Alcohol." By Warren De la Rue, Ph.D., F.R.S., and Hugo Müller, Ph.D., F.C.S. Communicated by Mr. De la Rue.

(Abstract)

In this communication the authors give an account of a new alcohol homologous with benzylic alcohol ($C_{14} H_8 O_2$) which they have found occurring in the state of a natural acetic ether in the exudation from an Australian plant known as the *Ficus rubiginosa*.

This acetic ether, for which they propose the name of Acetate of Sycoceryl, constitutes about 14 per cent. of the crude resin; the remainder consisting principally of an amorphous resin which they name Sycoretin.

The different degree of solubility of the various constituents in alcohol, afforded the means of the separation of the one from the other; none of them present any remarkable properties except the new ether, so that the authors have devoted their attention mainly to the working out of the chemical relations of this substance

Acetate of sycoceryl, having very characteristic properties, could be readily obtained in beautiful crystals, but some difficulty occurred in obtaining it absolutely pure, on account of the presence of a parasitical body which accompanied it constantly in solution, and always crystallized upon it. At last means were found of removing the latter substance by dissolving out the acetate of sycoceryl with ether The per-centage composition of this parasitical body was found to be—

Carbon	76 56
Hydrogen	12 30
Oxygen	11 24

but it existed in too small a quantity to admit of its true chemical relations being made out

Acetate of sycoceryl gave on analysis the following per-centages as the mean result of two accordant analyses —

Carbon	79·09
Hydrogen	10 28
Oxygen	10 63

These numbers agree well with those required by the formula $C_{40} H_{32} O_4$ based upon experimental evidence.

Acetate of sycoceryl, when acted upon by sodium-alcohol, yielded acetic acid and a beautiful crystalline body resembling caffeine or asbestos; this proved to be a new member of the benzylic alcohol series having the composition $C_{36} H_{30} O_2$, which requires the following per-centage quantities ·—

		Mean of two analyses.
Carbon	82·44	82 39
Hydrogen	11·45	11·38
Oxygen	6 11	6 23

The authors, by acting with chloride of benzoyl on sycocerylic alcohol, obtained the corresponding benzoate of sycoceryl, and by employing chloride of othyl (acetyle), have prepared the acetate of sycoceryl which was identical with the original crystalline constituent of the resin.

By treating sycocerylic alcohol with nitric acid, an acid was procured which appears to be sycocerylic acid.

The products of the action of chromic acid on sycocerylic alcohol, were a white crystalline neutral substance and a body crystallizing in large flat prisms. The latter appears to be the sycocerylic aldehyde.

II. "Analytical and Synthetical Attempts to ascertain the cause of the diffefences of Electric Conductivity discovered in Wires of nearly pure Copper." By Professor WILLIAM THOMSON, F.R.S. Received December 22, 1859.

Five specimens of copper wire No. 22 gauge, out of a large number which had been put into my hands by the Gutta Percha Company to be tested for electric conductivity, were chosen as having their conductivities in proportion to the following widely different numbers, 42, 71·3, 84·7, 86·4, and 102, and were subjected to a most careful chemical analysis by Professor Hofmann, who at my request kindly undertook and carried out what proved to be a most troublesome investigation. The following report contains a statement of the results at which he arrived:—

"Royal College of Chemistry,
March 10th, 1858.

"SIR,—I now beg to communicate to you the results obtained in the analysis of the several varieties of copper wire intended for the use of the Transatlantic Telegraph Company, which you forwarded to me for examination.

"I have limited the inquiry to a *minute qualitative* analysis of the wires, to a very accurate determination of the amount of copper, and an approximative determination of the amount of oxygen. The qualitative analysis has been repeated several times with as considerable quantities as the amount of material at my disposal permitted. The *quantitative* determinations of the copper have been made with particular care, and after a lengthened scrupulous inquiry into the limit of accuracy of which the method employed is capable, I am convinced that the true per-centages of copper cannot be more than 0·1 per cent. either above or below the means of the determinations, the details of which I give you in the Appendix.

"The following Table contains the results furnished by analysis:—

Conductivity of the wire, in relative measure*	42	71 3	84 7	86 4	102
Qualitative analysis	Copper Iron Nickel Arsenic Oxygen	Copper Iron Nickel Oxygen	Copper Iron Nickel (doubtful) Oxygen	Copper Iron Nickel (doubtful) Oxygen	Copper Iron Oxygen
Per-centage of copper	98 76	99 20	99 53	99 57	99 90
Amount of impurities	1 24	0 80	0 47	0 43	0 10
	100 00	100·00	100·00	100·00	100 00

"Since it appeared probable that the extraordinary difference in the conductivity of the several specimens was due rather to non-metallic impurities than to metallic admixtures, careful experiments were made in every case for the detection of sulphur In none of the specimens was it possible to discover the slightest trace of sulphur Qualitative experiments having established on the other hand the presence of oxygen, probably in the form of suboxide of copper in every one of the specimens, an attempt was made to ascertain the quantities by determining the loss which the wire after rolling suffered when heated in an atmosphere of hydrogen, and by simultaneously estimating the quantity of water formed

"In this experiment, the details of which are given in its Appendix, the following numbers were obtained·—

Conductivity	42	71 3	84 7	86 4	102
Percentage of Oxygen	0·067	0·119	0 172	0·159	0 193

"Unfortunately the same reliance cannot be placed upon these numbers as upon the preceding ones, since the method employed involves many sources of error, and want of material precluded the possibility of repeating the experiments

"From the preceding analysis, it is obvious that the amount of impurities in the several specimens examined is small, varying as it does between 0·10 and 1·24 per cent The number of foreign constituents also is comparatively small I should, however, state that the analytical results which I have given do not exclude the presence of exceedingly minute quantities, even of other metals which might have been detected if larger quantities of copper could have been submitted to analysis Some years ago, Max Duke of Leuchtenberg* examined the black precipitate formed at the anode in the electrotype process, during the decomposition of sulphate of copper by the galvanic current In this precipitate, of which considerable quantities accumulate by the gradual solution of large quantities of copper passing through the process, he found the following constituents :—

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* I have since found $10^{-2} \times 131\frac{1}{2}$ as the factor to reduce from this to absolute measure Thus the conductivities of the five specimens are respectively 55·2, 95 3, 111·4, 113 6, 134·1, in terms of one one thousand millionth of the British absolute unit —W T

Antimony	9 22	Iron	0 30	Oxygen	24 82
Arsenic	7 40	Nickel	2 26	Sulphur	2·46
Platinum	0 44	Cobalt	0 86	Selenium	1 27
Gold	0 98	Vanadium	0·04	Sand	1·90
Silver	4 54	Tin	33 50		
Lead	0·15	Copper	9 24		

"Of these constituents, the ten first metals were obviously derived from the copper, in which they could have been scarcely detected unless by this accumulative process Of the remainder of the constituents, the tin in a great measure is derived from the solderings.

"The results obtained in the analysis of the copper wires which you forwarded to me, appear to establish one fact in a satisfactory manner, viz that the diminution of conductivity observed in certain specimens of copper is due to the presence in these specimens of a certain amount of foreign matter, and not, as it has been supposed, to a peculiar change in the physical condition of the metal, for in the specimens analysed the conductive power rises in the same order as the total amount of impurities diminishes

"I have, &c.,

(Signed) "A. W. Hofmann."

"*Professor William Thomson, F.R S, &c*"

It appears therefore that in the case of these four specimens, the electric conductivity is in order of purity of the copper, but yet that only extremely small admixtures of other substances are to be found even in those which have but half the conductivity of the best.

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* Petersburgh Acad Bull vii p 218.

On the other hand, I have found by experimenting on artificial alloys, that comparatively large admixtures of lead, iron, silver, and zinc seem to produce sometimes improvement, sometimes little or no sensible influence, and sometimes (as in the case of zinc) an injurious effect on the conductivity of specimens of pure electrotype copper from which the alloys were made The largeness of the proportion of other metal required to produce any considerable deterioration in comparison with that of the whole amount of impurities which Professor Hofmann's investigation demonstrates in specimens of low quality as to conductivity, is worthy of remark, and seems to indicate that this low quality must be due to other than metallic impurities

The great difference between the conducting qualities of two specimens of electrotype copper, from which two series of alloys were separately prepared, seems also to indicate some as yet undiscovered cause, as operative in general I am assured by Messrs Matthey and Johnson, by whom all the alloys were prepared, that similar methods were followed and equal care bestowed to ensure purity in the two cases

The results of my measurements of conductivity are shown in the following Tables :—

TABLE I —Two Series, Nos 1–10 and Nos. 1–32, of Specimens prepared by Messrs Matthey and Johnson from pure electrotype copper, and the same alloyed with other metals, as specified

No. of Spec.	Specification of compound	Specific conductivity
	SERIES I	
1	Pure copper	138 5
2	Pure copper alloyed with 25 per cent. silver	138 5
3	Pure copper alloyed with 13 per cent. silver	139 5
4	Pure copper alloyed with ·25 per cent lead	144
5	Pure copper alloyed with 13 per cent lead	146
6	Pure copper alloyed with 25 per cent tin	131
7	Pure copper alloyed with 13 per cent tin	133
8	Pure copper alloyed with 80 per cent zinc	125
9	Pure copper alloyed with 40 per cent. zinc	120 5
10	Pure copper alloyed with 1 40 per cent zinc	103

TABLE I—*continued.*

No of Spec	Specification of compound	Specific conductivity
	SERIES II	
1	997·5 copper+2·5 silver	69·8
2	998·7 copper+1·3 silver	117·7
3	997·5 copper+2·5 lead	94·5
4	998·7 copper+1·3 lead	105·8
5	997·5 copper+2·5 tin	91·6
6	998·7 copper+1·3 tin	116·9
7	999 copper+1 silver	126·7
8	999 copper+1 lead	134·2
9	999 copper+·5 lead+·5 silver	128·0
10	Equal parts of 1 and 3	89·3
11	997·5 copper+2·5 iron	120·7
12	998·7 copper+1·3 iron	113·7
13	1000 copper+2·5 protoxide of copper	122·5
14	1000 parts of 3 &+2·5 protoxide of copper (too brittle to test)	
15	1000 parts of 4 &+2·5 protoxide of copper	119·7
16	997·5 copper+2·5 zinc	108·9
17	995 copper+2·5 lead+2·5 zinc	85·1
18	995 copper+2·5 lead+2·5 iron	131·5
19	998·7 parts of 11+1·3 lead	135·0
20	997·5 parts of 11+2·5 zinc	77·6
21	998·7 parts of 11+1·3 zinc	95·2
22	997 parts of 11+1·3 lead &+1·3 zinc	117·6
23	992 copper+8 zinc	118·9
24	996 copper+4 zinc	117·0
25	986 copper+14 zinc	80·2
26	982 copper+18 zinc	102·3
27	994 copper+6 zinc	109·5
28	980 copper+20 aluminium	44·0
29	990 copper+10 aluminium	128·7
30	995 copper+5 aluminium	122·5
31	997 copper+3 aluminium	130·2
32	Pure copper, from which all the above were made	120·9

TABLE II—First Series (10 specimens) arranged in order of conductivity

No of Spec	Specification of compound	Specific conductivity
5	Pure copper alloyed with 13 per cent of lead	146
4	Pure copper alloyed with 25 per cent of lead	144·5
3	Pure copper alloyed with 13 per cent of silver	139·5
2	Pure copper alloyed with 25 per cent. of silver	138·5
1	Pure copper	136·5
7	Pure copper alloyed with 13 per cent of tin	133
6	Pure copper alloyed with 25 per cent. of tin	131
8	Pure copper alloyed with 80 per cent of zinc	125
9	Pure copper alloyed with 40 per cent. of zinc	120·5
10	Pure copper alloyed with 1·40 per cent of zinc	108

Table III —Second Series (32 specimens) arranged in order of conductivity.

No. of Spec	Specification of compound with manufacturers' description of mechanical quality of wire	Specific conductivity
19	998·7 of No 11+1·3 lead *fair*	135·0
8	999 copper+1 lead fair	134·2
18	995 *copper+2·5 lead+2·5 iron very good*	131·5
31	997 copper+3 aluminium good	130·2
11	997·5 copper+2·5 *iron not very good*	129·7
29	990 copper+10 aluminium good	128·7
9	999 copper+·5 lead+·5 silver *rather better than No 8*	128·0
7	999 copper+1 silver fair	126·7
13	1000 copper+2·5 *protoxide of copper very bad*	122·5
30	995 copper+5 aluminium very good	122·5
32	Pure copper *very good*	120·9
15	1000 of No 4+2·5 protoxide of copper better than No 14, but not good	119·7
23	992 copper+8 zinc first-rate alloy	118·9
2	998·7 copper+1·3 silver fair, but rather frangible	117·7
22	997·5 of No 11+1·3 lead+1·3 zinc very good indeed	117·6
24	996 copper+4 zinc moderately good	117·0
6	998·7 copper+1·3 tin perhaps not quite as good as No 5	116·9
12	998·7 copper+1·3 iron frangible	113·7
27	994 copper+6 zinc good	109·5
16	997·5 copper+2·5 zinc first rate alloy	108·9
4	998·7 copper 1·3 lead rather better than No 4	105·8
26	982 copper 18 zinc very good	102·3
21	998·7 of No 11+1·3 zinc very fair	95·2
3	997·5 copper+2·5 lead good, but requires care	94·5
5	997·5 copper+2·5 tin much the same as Nos 3 and 4	91·6
10	Equal parts of Nos 1 and 3 bad, frangible	89·3
17	995 copper+2·5 lead+2·5 zinc very good	86·1
25	986 copper+14 zinc first-rate alloy	80·2
20	997·5 of No 11+2·5 zinc very fair	77·6
1	997·5 copper 2·5 silver fair, but rather frangible	69·8
28	980 copper+20 aluminium not very good	44·0
14	1000 parts of No 3+2·5 protox copper, almost undrawable (too brittle to test)	

The alloys numbered 14 and 15 were prepared with a view to testing the possible effect of a suboxide of copper mixed or combined with the mass Although they do not seem worse than others of nearly the same metallic composition, it cannot be considered that they demonstrate that oxygen exercises no influence, as the portion of oxide introduced may have been reduced in the melting ; and indeed it is quite possible that some accident in the melting may possibly give rise to *oxidation* to a greater or less degree, and may cause some of the irregularities and uncertainties which have been observed. On this I may remark, that although I have found that no mechanical alteration by hammering, twisting, &c produces any considerable effect of the conductivity of one piece of solid copper, I have not yet found whether or not specimens either good or bad retain their specific qualities after melting.

I may add, that it will be of great importance to ascertain the laws of variation of conductivity with temperature, of different specimens of nearly pure copper differing largely in conductivity. I have hitherto used standards of copper wire in all the relative determinations of conductivity which I have made for different commercial specimens and artificial alloys of copper, and before I found the very large differences of conductivity shown in this and in my preceding communication to the Royal Society (June 15, 1857), it seemed natural to suppose that the relation between specimen and standard would remain constant, or nearly constant, when the temperatures of the two are varied to the same extent Now, however, it seems scarcely probable that this can be the case, and a rigorous experimental examination of the influence of temperature becomes necessary

P.S April 11, 1860—I append the following extract from evidence which I gave on examination before the Government Committee on submarine telegraphs on the 17th December, 1859, as it bears directly on the subject of the preceding article, and shows what degree of weight may in my opinion be attached to the synthetical attempts which have been described

(*Chairman.*) Question 2458. Soon after you became a Director of the Atlantic Telegraph Company, was your attention directed to the conductivity of copper?—Yes.

2459. You instituted a series of experiments, did not you, to determine the variation of this quality in different samples of copper?—A number of samples of copper were, at my request, put into my hands for the purpose of measuring their conductivity in consequence of my having accidentally noticed differences greater than I expected in the conducting power of one or two samples which I had had previously.

2460. Will you be good enough to state the general results at which you ultimately arrived, and your modes of experimenting?—My modes of experimenting did not differ materially from the methods which had been followed by certain other experimenters, especially in Germany, and were in reality all based on Professor Wheatstone's invention of a beautiful method for comparing resistances, to which I have frequently referred as Professor Wheatstone's electric balance.

2461 What were the results at which you arrived?—That different specimens chosen at random from the stock supplied for manufacture differed immensely in conducting power.

2462 Although nominally the same quality of copper?—Yes, although nominally the same quality of copper All those specimens of wire were supposed to be of the very best quality, the only copper supposed to be good being that which admitted of being drawn into wire suitably for the purpose. A good mechanical quality was necessary to prevent frequent fractures in the wire-drawing, and to understand that, I should say that hanks in unbroken lengths amounting to a large mass were always required, the worse metal being found to break before it could be drawn into a hank of a certain size The mechanical qualities seem to have been satisfactory, but no suspicion whatever was entertained that there were also large differences in electric conducting power. W Weber had many years before pointed out considerable differences in different specimens of copper wire which he had tested. I found differences much exceeding those, and I did not, as I expected, find any approximation to a uniform average among the different specimens tested, some specimens I found nearly double in their conducting power, compared with others, reckoned according to the weight and length, allowing for the variations of gauge Calling the best specimen which I had in the summer of 1857, 100, I found many specimens standing at 60 in specific conductivity, many standing at 50, many standing at 80, a few above 90, and so far as I can recollect, the average of a large number of specimens that I then examined may have stood between 60 and 70, but I consider the statement of such an average to be of no value, it is so much a matter of chance If I had received a dozen specimens of a low quality below the average, or if I had chanced to receive a dozen specimens of a higher quality, the average would have been so much the lower or the higher I never had an opportunity of measuring the conductivity of 200 or 300 miles of submarine cable, such alone would have given me exact information as to the average for that portion of cable I may mention that a month or two later, still in the summer of 1857, I received specimens of wire which were in stock for submarine telegraphs,—for some of the Mediterranean telegraphs, I believe,—which stood as low as 43 on that scale, and, lastly, I may mention that I have since met with specimens standing 2 or 3 per cent above the 100, and an artificial alloy, which I had prepared, stood, so far as I can estimate, as high as 111

2463. What was that alloy?—The alloy consisted, so far as I can recollect, of copper and 13 per cent of lead I have made experiments upon a series of alloys, in all about 43 or 44, and have recently repeated the examination so as to arrive at accuracy, within certain limits, and I expect, immediately, to be able to communicate to the Royal Society, for publication, the results A few months ago I sent a provisional list of the specimens, showing the relative conductivity of those alloys, but, possibly, requiring correction as to the absolute conductivity stated That list was communicated to Mr Latimer Clarke, and, I believe, a copy of it was laid before the Committee

2464 (*Professor Wheatstone*) Were you quite certain that you employed pure copper in your experiments?—I could not be quite certain

2465 The copper might be alloyed with other things than metals, is it not very probable that it might contain some suboxide, and that the mixing of lead afterwards with it might have reduced the suboxide, and therefore have given it a higher conducting power on that account?—That is possible. I cannot say that I am at all satisfied that the experiments which I have made point out distinctly the relation between the ascertained chemical combination and conductivity I may mention that one of my alloys was made with a suboxide melted with the copper, but the uncertainty of the process of melting the suboxide and the uncertainty as to how much of the oxidation may have disappeared in the melting, prevented me from attributing much weight to the experiment

2466 (*Chairman*) What was the result with that alloy, was it a low result, or a high result?—A moderate result, not a low result.

2467 But not a high one?—A somewhat high result; but I may mention that in one series the highest conductivity was found with a mixture of lead and iron, fractions of a per cent. of lead, and fractions of a per cent of iron mixed with pure copper gave a higher conductivity than a nominally pure copper, with which the alloys were prepared I must mention further, that in two series the alloys, both prepared by Messrs Matthey and Johnson, and as I have been assured with equal care, gave results presenting considerable discrepancies, the conductivity of the pure copper in the first stood high, nearly agreeing with the 100 of my first scale, the pure copper of the second series fell considerably below that limit On this account it appears that even pure copper, carefully prepared by the electrotype process, does not always give us results which show perfectly in point of conductivity, but to make such experiments in a satisfactory manner, it would be necessary to have a thorough chemical investigation, both synthetical and analytical, of the metals used, such a thorough investigation I have not been able to carry out, in consequence of the large expense which it would entail I may mention that Mr Matthiessen has gone through a series of experiments on alloys, of which the chemical composition has been ascertained with all possible accuracy, and has, I believe, arrived at highly important results relative to electrical conductivity I have been in communication with him, and have supplied him with a specimen of one of my standards He mentions to me that he has obtained specimens conducting better to a considerable extent than the 100 of my first scale In that respect he has confirmed what I have myself ascertained, having myself found specimens as high as 111 on that scale. A number of alloys of definite chemical composition, prepared with great care by Mr Calvert of Manchester, and already tested by him for thermal conductivity and for mechanical properties, have been put into my hands, in order that I may measure their electric conductivities. I hope soon to be able to obtain and publish results for this series of alloys.

III "On a new Method of Substitution, and on the formation of Iodobenzoic, Iodotoluylic, and Iodanisic Acids." By P Griess, Esq Communicated by Dr Hofmann. Received January 3, 1860.

In a previous notice* I have pointed out the existence of a new class of nitrogenous acids which are generated by the action of nitrous acid on the amidic acids of the benzoic group, the change consisting in the substitution of one equivalent of nitrogen for three equivalents of hydrogen in two molecules of the amidic acid

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* Proceedings of the Royal Society, vol. ix p 594.

$$\underbrace{C_{28}H_{14}N_2O_8}_{\text{Two eqs benzamic acid}} + NO_3 = 3HO + \underbrace{C_{28}H_{11}N_3O_8}_{\text{New acid.}}$$

Under the influence of various agents these new acids undergo remarkable changes, amongst which the transformation produced by the mineral acids deserves to be particularly noticed If the acid $C_{28}H_{11}N_3O_8$ be gently heated with strong hydrochloric acid, nitrogen gas is evolved, the yellow colour of the original acid disappears, and a red body separates, which may be separated by filtration and purified by treatment with animal charcoal. Both the physical properties and the analysis of the substance thus obtained, prove it to be pure *chlorobenzoic acid* The hydrochloric mother-liquor on evaporation deposits crystals of the *hydrochlorate of benzamic acid*

$$C_{28}H_{11}N_3O_8 + 2HCl = C_{14}(H_5Cl)O_4 + C_{14}(H_5H_2N)O_4, HCl + N_2$$

To render intelligible this transformation, the acid $C_{28}H_{11}N_3O_8$ may be viewed as a double acid corresponding to two molecules of water,

$$\left.\begin{array}{l} C_{14}(H_3N'_2)O_2 \\ C_{14}(H_4H_2N)O_2 \\ \qquad\qquad\quad H_2 \end{array}\right\} O_4 = C_{14}(H_4N'_2)O_4 + C_{14}(H_5H_2N)O_4,$$

and splitting under the influence of hydrochloric acid into the two groups $C_{14}(H_4N'_2)O_4$ and $C_{14}(H_5H_2N)O_4$, in the first of which the two equivalents of monatomic nitrogen are replaced by hydrochloric acid, producing $C_{14}(H_5Cl)O_4$, while the second simply combines with hydrochloric acid, producing hydrochlorate of benzamic acid. It deserves to be mentioned that the acid $C_{28}H_{11}N_3O_8$ may be derived also from two equivalents of hydrated oxide of ammonium, when its formula assumes the following shape :—

$$\left.\begin{array}{r} [(C_{14}H_4O_2)_2{}''N'''HN_2]'' \\ H_2 \end{array}\right\} O_4$$

Further experiments are necessary to decide which of these two formulæ deserves the preference.

Iodobenzoic Acid, $C_{14}(H_5I)O_4$

This substance is produced by a process similar to that which furnishes the chlorobenzoic acid, viz. by the action of hydriodic acid on the acid $C_{28}H_{11}N_3O_8$,—beautiful white plates resembling benzoic acid, easily soluble in alcohol and in ether and difficultly soluble in water. Iodobenzoic acid is remarkable for its great stability; even fuming nitric acid fails to expel the iodine, and transforms the substance simply into nitro-iodobenzoic acid. The silver salt of iodobenzoic acid is a white amorphous precipitate containing $C_{14}(H_4IAg)O_4$.

Iodotoluic Acid, $C_{16}(H_7I)O_4$

This acid is formed from the analogous nitrogenous acid in the toluic series, according to the equation

$$C_{32}H_{15}N_3O_8+2HI=C_{16}(H_7I)O_4+C_{16}(H_7H_2N)O_4, HI+N_2$$

It crystallizes in white plates of a pearly lustre, which in their chemical and physical properties are very similar to iodobenzoic acid.

Iodanisic Acid, $C_{16}(H_7I)O_6$

is obtained by the action of hydriodic acid upon the nitrogenous acid $C_{32}H_{15}N_3O_{12}$,

$$C_{32}H_{15}N_3O_{12}+H_2I_2=C_{16}(H_7I)O_6+C_{16}(H_7H_2N)O_6, HI+N_2$$

Exceedingly small, nearly white needles, almost insoluble in boiling water, very soluble in alcohol and in ether.

The new method of substitution, by which the described products were obtained, although less direct than the ordinary processes, promises nevertheless to adapt itself to several cases of special interest. I am at present engaged in pursuing these experiments, with the view of producing fluo- and cyano-benzoic acids and their homologues, which have never been obtained.

The experiments which I have described were performed in Professor Hofmann's laboratory.

February 16, 1860

Sir BENJAMIN C BRODIE, Bart., President, in the Chair

The following communications were read —

I "Description of an Instrument combining in one a Maximum and Minimum Mercurial Thermometer, invented by Mr. James Hicks" By Balfour Stewart, Esq. Communicated by J P Gassiot, Esq Received Feb 7, 1860

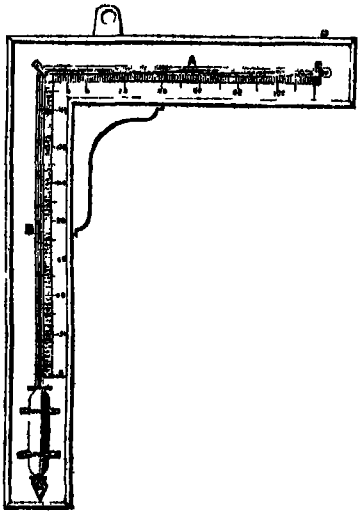
About a fortnight since, Mr. James Hicks, the intelligent foreman of Mr. L P Casella, Optician, called at Kew Observatory with an instrument of the above description, for the purpose of having it compared with the ordinary maximum and minimum thermometers. This comparison proving very satisfactory, and the principle of the instrument commending itself to Dr Robinson, Mr Gassiot, Professor Walker, and several other scientific men who examined it, Mr Gassiot requested me to write a short description of it, which he thought might be of interest to the Royal Society For many particulars of this description, I am indebted to Mr Casella and Mr Hicks, who furnished me with details regarding the construction of the instrument

Its chief advantage consists in its furnishing us with a mercurial minimum thermometer, no serviceable instrument of this description having hitherto been made At the same time it is also capable of being used as a mercurial maximum thermometer

The principle of the instrument is briefly as follows :—

It has a cylindrical bulb nearly 3½ inches long and half an inch in diameter, filled with mercury. This gives a bore nearly $\frac{1}{20}$th of an inch wide, and a scale on which 1° Fahr corresponds to about $\frac{1}{20}$th of an inch. When the graduation has reached 150° Fahr or so, both the tube and the scale are made to assume a position at right angles to that which they occupied previously, so that the first portion of the thermometer being vertical, the second will be horizontal The numbers on the horizontal scale are not, however, in continuation of those on the vertical; for in the instrument from which this account is taken, while 150° is the highest division on the vertical scale, the first on the horizontal is −10°, the next 0°, the 3rd 10°, and so on. The reason of this method of graduation will immediately appear.

Above the mercury there is a small quantity of spirits of wine, which extends some distance into the horizontal tube. The quantity of this, and the graduation, correspond in such a manner, that the extreme end of the spirit column denotes the same degree of temperature as the mercury The remainder of the horizontal tube is filled with air There are two moveable indices in the spirit column, one in the vertical tube, the other in the horizontal, each about half an inch long. The former, B, consists of a fine steel magnet enclosed in glass. This forms the body of the index At either extremity there is a head of black glass, similar to that which occurs in the index of an ordinary minimum thermometer A fine hair is tied round the neck of this index, between the body and the upper head, and it is made to hang down by the side, so that by its elastic pressure against the tube, the index may be kept in its place, notwithstanding its verticality The index in the horizontal tube A, is in all respects similar to that of an ordinary minimum thermometer



Let us now suppose the instrument fixed in its position, the first part of the stem being vertical In order to adjust it, we must first bring the vertical index into contact with the upper extremity of the mercurial column To do this, let us take two small but strong horseshoe magnets, and lay the one above the other, so that the poles of the one shall overlap to a small extent the corresponding poles of the other Bring the magnets up to the index in such a manner, that, while the poles of the one bear against the side of the glass tube, the overlapping poles shall lie over the tube so as to be in front of the index · the index will now follow the motion of the magnets, and it may thus be brought down to the surface of the mercury In order to bring the horizontal index to the extremity of the spirit column, all that is necessary is to incline the horizontal tube a little downwards by pressing on the end

The indices being now set and the instrument in adjustment, let us suppose the temperature to rise, the mercurial column will push the vertical index up, but this index will remain in its place when the mercury again falls, and will therefore denote the maximum temperature reached On the other hand, let us suppose the temperature to fall. The mercury in falling is followed by the spirit column propelled by the air behind it The spirit column, again, will, on its edge coming in contact with the end of the horizontal index, draw the index with it into a position, where it will remain when the mercury again rises. This index will therefore register the extreme minimum point which the spirit column has reached; but by the principle of graduation, this will correspond with the minimum point reached by the mercurial column.

Let us now suppose that a small portion of the spirit column has become separated, and lodged itself in the extremity of the tube. The principle of graduation will immediately enable us to discover this, by a want of correspondence being produced in the readings of the mercurial and of the spirit column If, for instance, before the separation, the mercury read 50°, and the horizontal extremity of the spirit column also 50°, it is clear that, after the abstraction of spirits has taken place, the horizontal column will read lower.

We have thus a check upon this possible source of error, which we have not in the ordinary minimum thermometer. Indeed, it is to all intents a mercurial minimum thermometer that we are now describing, the spirits serving merely as a vehicle for the indices It will be remarked, that, were both columns capable of acting in a horizontal position, there would be no necessity for the bend, and the instrument would be more portable, but in this position it is found that there is danger of the spirits becoming mixed with the mercury, and thus interfering with the action of the instrument. Should this ever be brought about by travelling, or any other cause, a smart jerk or two of the instrument will join the separated columns and put all right.

The instrument is thus constructed.—The vertical tube, including the bulb, is first made and filled with mercury to the proper height, and the magnetic index is introduced, then the horizontal tube is joined, and the spirits of wine and the horizontal index are introduced. The bulb is then placed in a freezing mixture, in order that the mercury may retreat as far as possible, followed by the spirits of wine The tube is then sealed, care being taken that the bore shall end in a small rounded chamber, for if pointed, some of the spirits would be apt to lodge there, whence it would be difficult to remove it The object of cooling the bulb before sealing off, is that we may have as much air in the tube as possible, for its pressure, as already mentioned, enables the spirits to follow the mercury when the latter falls

To graduate the instrument, set it with the mercurial stem horizontal in melting ice, then point off the extremity of the mercurial, and also of the spirit column as corresponding to 32° Fahr Perform a similar operation in water at 42°, 52°, 62°, &c, and also in freezing mixtures down to zero, or lower if necessary

In conclusion, if used as a wet-bulb thermometer, this instrument will give us the maximum and minimum temperatures of evaporation obtained under precisely the same circumstances

II "On the Expansion of Metals and Alloys" By F Crace-Calvert, Esq, F R S, and G. Cliff Lowe, Esq Communicated by Mr. Calvert. Received December 1, 1859.

[Abstract]

One of us having been engaged for some time in investigating several of the properties of pure metals, it was thought desirable to take advantage of having pure metals at our disposal, together with a series of definite alloys of those metals, to determine their rate of expansion And we were encouraged in pursuing this course of investigation, by finding that several of the authors who had previously published tables of the expansion of metals differed widely in their results. These discrepancies, having reference to some of the metals most extensively used, might, we thought, be due either to the method employed, or to the fact that metals of different degrees of purity had been experimented upon Therefore, being sure of the purity of the metals that we intended to employ, we had recourse to a method the accuracy of which we trust will appear satisfactory

Owing to the difficulty of obtaining the metals in a pure state in large quantities, we found it necessary to employ square bars, having a length of 60 millimetres by 10 millimetres of diameter We therefore devised a process to determine with accuracy the expansion of such short bars. This, we believe, we have effected, as our apparatus will easily indicate an expansion amounting to the 50,000th of an inch, or about the 2000th part of a millimetre

Omitting the description of our apparatus and of the details of our operations, which would be long for this abstract of our results, we give here a Table of the general results obtained with the following metals.—

	Divisions of the scale read off in 26,000ths of an inch in a rising temperature from 10° to 90°				Divisions of same scale read off in cooling from 90° to 10°				Mean for 100° C calculated from those means and corrected for expansion of vessel &c, by deducting 20
	1st	2nd	3rd	Mean	1st	2nd	3rd	Mean	
Cadmium	174	171	172	172·3	176	173	172	173·7	196·2
Lead	155	156	157	156	161	160	159	160·0	177·5
Tin	142	142	145	143	147	148	147	147·3	161·5
Aluminium	120	120	120	120	122·5	121	122	121·8	131·1
Forged zinc	119	121	120	120	120	119	120	119·7	129·8
Silver ..	110	109	109·5	109·5	111·5	110	110	110·5	117·5
Copper (pure) cast	106	105·5	106	105·8	103	103·5	107	104·5	111·4
Copper (pure) hammered	99	99	99	99	101	99	100	100	104·4
Gold .	81	80·5	..	80·7	81·5	81		81·3	81·3
Bismuth	78	77	77·5	77·5	81·5	80	80	80·5	78·8
Wrought iron	69	72	73	71·3	73·5	72	72·5	72·7	70·0
Cast iron	67	68·5	68·5	68·0	68	70·5	70·5	69·7	66·1
Steel (soft) .	66·5	62·0	63	63·8	66·5	65	66·5	66	61·1
Antimony	63	62		62·5	63	62	.	62·5	58·1
Platinum	57·5	.		57·5	58		.	58·0	52·2

From the above observations we deduce the following Table of coefficients of linear expansion from 0° to 100° —

Cadmium (pure)	0·00332
Lead (pure) ..	0·00301
Tin (pure) . ..	0·00273
Aluminium (commercial) ..	0·00222
Zinc, forged (pure)	0·00220
Silver (pure) .. .	0·00199
Gold (pure) .. .	0·00138
Bismuth (pure) . . .	0·00133
Wrought iron	0·00119

Cast iron	0 00112
Steel (soft)	0 00103
Antimony (pure)	0 00098
Platinum (commercial)	0 00068

On comparing these coefficients with those found by previous experimenters, we find that they agree very closely in those cases where commercial metals have been employed. But when we come to those metals which we employed in a pure state, such as lead, tin, zinc, silver, copper, bismuth, antimony, cadmium, and gold, we find a marked difference, which we attribute to our experiments having been made with pure metals, and we are confirmed in this view by several series of experiments made with impure or commercial metals

We give in our paper several series of experiments which prove that, as for conductibility of heat by bodies, their molecular condition exercises the greatest influence on their expansion For example, we have found that the same bar of steel gives, according to its degree of tempering, the following ratios of expansion —

	Raising temperature from 10° to 90°				Cooling temperature from 90° to 10°				Mean calculated and corrected for a bar 60 mm for 100°
	1st	2nd	3rd	Mean	1st	2nd	3rd	Mean	
Steel bar as purchased	111	112	111 5	111 5	113	113	115	113 7	64 6
Steel bar at maximum of softness	107	108	107 5	107 5	107	112	111	110	62 5
Same bar at maximum of hardness	141	145	140	142	138	139	139	136 7	84 0

The influence of the molecular state of bodies is also clearly illustrated in the class of compounds or carbonates of lime —

	Rates of expansion from 0° to 100°.
Chalk	**19·6**
Lithographic stone	**45·0**
Stalactite	**67·0**
Marble	**71·0**

Influence of Crystallization

Crystallization influences the expansion of bodies as it does their power to conduct heat, thus, the same zinc cast horizontally or vertically has not only a different crystallized structure, but also expands in a different ratio

	Raising temperature 10° to 90°					Cooling temperature 90° to 10°				Mean for 100° less correction
	1st	2nd	3rd	4th	Mean	1st	2nd	3rd	Mean	
Zinc cast vertically	224	226	227	226	226	232	233	234	233	266 9
Zinc cast horizontally	187	186 5	187		186 8	190 5	193	192 5	192	216 7

We have also examined the ratio of expansion in several series of alloys made in multiple and definite proportions, but shall give here only one series as illustrating our results

Copper and Tin

	10° to 90°				Mean for 100° less correction					Mean for 100° less correction
	1st	2nd	3rd	Mean		1st	2nd	3rd	Mean	
5Sn 90 27 1Cu 9 73	127	124	124	125	136 2	129	124	..	126 5	138 1
4Sn 88 14 Cu 11 86	122	122	.	122·0	132 5	127	126		126 5	138 1
3Sn 84 79 Cu 15 21	119	119 5		119 2	129 5	123	122	123 5	122 8	133 5
2Sn 78·79 Cu 21·21	118	118		118	127 5	117	117		117	126·2
Sn 65 02 Cu 34 98	109 5	111 5	.	110 5	118 1	110 5	110 5		110 5	118 1
Sn 48 17 2Cu 51 83	111·0	111	.	111	118 7	113·5	112	.	112·7	120 8
Sn 34 21 3Cu 61 79	112	111		111·5	119 3	113	112	..	112 5	120·6
Sn 31·73 4Cu 68 27	102	105		103 5	109 3	106	105	.	105 5	111·8
Sn 27 10 5Cu 72·90	104	104		104	110	106	106	.	106	112 5
Sn 15 68 10Cu 84 32	101	101	.	101	106·2	101	101		101	106 2
Sn 11 03 15Cu 88·97	95	94·5	.	94 7	98·3	94 5	95		94 7	98·8
Sn 8 51 20Cu 91·49	97	99 5		98 2	102 7	99	99		99	103·7
Sn 6·83 25Cu 93 17	99	99	...	99	103 7	100	100	.	100	105

February 23, 1860

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "Measurement of the Electrostatic Force produced by a Daniell's Battery." By Professor William Thomson, F.R.S. Received January 21, 1860.

In a paper "On Transient Electric Currents," published in the Philosophical Magazine for June 1853, I described a method for measuring differences of electric potential in absolute electrostatic units, which seemed to me the best adapted for obtaining accurate results. The "absolute electrometer" which I exhibited to the British Association on the occasion of its meeting at Glasgow in 1855, was constructed for the purpose of putting this method into practice, and, as I then explained, was adapted to reduce the indications of an electroscopic * or of a torsion electrometer to absolute measure.

The want of sufficiently constant and accurate instruments of the latter class has long delayed my carrying out of the plans then set forth. Efforts which I have made to produce electrometers to fulfil certain conditions of sensibility, convenience, and constancy, for various objects, especially the electrostatic measurement of galvanic forces, and of the differences of potential required to produce sparks in air, under definite conditions, and the observation of natural atmospheric electricity, have enabled me now to make a beginning of absolute determinations, which I hope to be able to carry out soon in a much more accurate manner. In the meantime I shall give a slight description of the chief instruments and processes followed, and state the approximate results already obtained, as these may be made the foundation of various important estimates in several departments of electrical science

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* I have used the expression "electroscopic electrometer," to designate an electrometer of which the indications are merely read off in each instance by a single observation, without the necessity of applying any experimental process of weighing, or of balancing by torsion, or of otherwise modifying the conditions exhibited.

The absolute electrometer alluded to above, consists of a plain metallic disc, insulated in a horizontal position, with a somewhat smaller plane metallic disc hung centrally over it, from one end of the beam of a balance A metal case protects the suspended disc from currents of air, and from irregular electric influences, allowing a light vertical rod, rigidly connected with the disc at its lower end, and suspended from the balance above, to move up and down freely, through an aperture just wide enough not to touch it In the side of the case there is another aperture, through which projects an electrode rigidly connected with the lower insulated disc. The upper disc is kept in metallic communication with the case

In using this instrument to reduce the indications of an electroscopic or torsion electrometer to absolute electrostatic measure, the insulated part of the electrometer is kept in metallic communication with the insulated disc, while the cases enclosing the two instruments are also kept in metallic communication with one another A charge, either positive or negative, is communicated to the insulated part of the double apparatus. The indication of the tested electrometer is read off, and at the same time the force required to keep the moveable disc at a stated distance from the fixed disc below it, is weighed by the balance This part of the operation is, as I anticipated, somewhat troublesome, in consequence of the instability of the equilibrium, but with a little care it may be managed with considerable accuracy. The plan which I have hitherto followed, has been to limit the play of the arm of the balance to a very small arc, by means of firm stops suitably placed, thus allowing a range of motion to the upper disc through but a small part of its whole distance from the lower. A certain weight is put into the opposite scale of the balance, and the indications of the second electrometer are observed when the electric force is just sufficient to draw down the upper disc from resting in its upper position, and again when insufficient, to keep it down with the beam pressed on its lower stop. This operation is repeated at different distances, and thus no considerable error depending on a want of parallelism between the discs could remain undetected. It may be remarked that the upper disc is carefully balanced by means of small weights attached to it, so as to make it hang as nearly as possible parallel to the lower disc The stem carrying it is graduated to hundredths of an inch, and by watching it through a telescope at a short distance, it is easy to observe $\frac{1}{1000}$th of an inch of its vertical motion

I have recently applied this method to reduce to absolute electrostatic measure the indications of an electrometer forming part of a portable apparatus for the observation of atmospheric electricity In this instrument a very light bar of aluminium attached at right angles to the middle of a fine platinum wire, which is firmly stretched between the inside coatings of two Leyden phials, one occupying an inverted position above the other, experiences and indicates the electrical force which is the subject of measurement, and which consists of repulsions in contrary directions on its two ends, produced by two short bars of metal fixed on the two sides of the top of a metal tube, supported by the inside coating of the lower phial

The amount of the electrical force (or rather as it should be called in correct mechanical language, *couple*) is measured by the angle through which the upper Leyden phial must be turned round an axis coincident with the line of the wire, so as to bring the index to a marked position An independently insulated metal case, bearing an electrode projecting outwards, to which the body to be tested is applied, surrounds the index and repelling bars, but leaves free apertures above and below, for the wire to pass through it without touching it, and by other apertures in its sides and top, it allows the motions of the index to be observed, and the Leyden phials to be charged or discharged at pleasure, by means of an electrode applied to one of the fixed bars described above When by means of such an electrode the inside coatings of the Leyden phials are kept connected with the earth, this electrometer becomes a plain repulsion electrometer, on the same principle as Peltier's, with the exception that the index, supported by a platinum wire instead of on a pivot, is directed by elasticity of torsion instead of by magnetism, and the electrical effect to be measured is produced by applying the electrified body to a conductor connected with a fixed metal case round the index and repelling bars, instead of with these conductors themselves

This electrometer, being of suitable sensibility for direct comparison with the absolute electrometer according to the process described above, is not sufficiently sensitive to measure directly the electrostatic effect of any galvanic battery of fewer than two hundred cells with much accuracy Not having at the time arrangements for working with a multiple battery of reliable character, I used a second torsion electrometer of a higher degree of sensibility as a medium for comparison, and determined the value of its indications by direct reference to a Daniell's battery of from six to twelve elements in good working order. This electrometer, in which a light aluminium index, suspended by means of a fine glass fibre, kept constantly electrified by means of a light platinum wire hanging down from it and dipping into some sulphuric acid in the bottom of a charged Leyden jar, exhibits the effects of electric force due to a difference of potentials between two halves of a metallic ring separately insulated in its neighbourhood, will be sufficiently described in another communication to the Royal Society Slight descriptions of trial instruments of this kind have already been published in the Transactions of the Pontifical Academy of Rome*, and in the second edition of Nichol's Cyclopædia (article Electricity, Atmospheric), 1860

I hope soon to have another electrometer on the same general principle, but modified from those hitherto made, so as to be more convenient for accurate measurement in terms of constant units In the meantime I find, that, by exercising sufficient care, I can obtain good measurements by means of the divided ring electrometer of the form described in Nichol's Cyclopædia.

In the ordinary use of the portable electrometer, a considerable charge is communicated to the connected inside coatings of the Leyden phials, and the aluminium index is brought to an accurately marked position by torsion, while the insulated metal case surrounding it is kept connected with the earth. The square root of the reading of the torsion-head thus obtained measures the potential, to which the inside coatings of the phials have been electrified. If, now, the metal case referred to is disconnected from the earth and put in connexion with a conductor whose potential is to be tested, the square root of the altered reading of the torsion-head required to bring the index to its marked position in the new circumstances measures similarly the difference between this last potential and that of the inside coatings of the phials Hence the excess of the latter square root above the former expresses in degree and in quality (positive or negative) the required potential This plan has not only the merit of indicating the quality of the electricity to be tested, which is of great importance in atmospheric observation, but it also affords a much higher degree of sensibility than the instrument has when used as a plain repulsion electrometer, and, on account of this last-mentioned advantage, it was adopted in the comparisons with the divided ring electrometer On the other hand, the portable electrometer was used in its least sensitive state, that is to say, with its Leyden phials connected with the earth, when the comparisons with the absolute electrometer were made.

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* Accademia Pontificia dei Nuovi Lyncei, February 1857.

The general result of the weighings hitherto made, is that when the discs of the absolute electrometer were at a distance of twenty hundredths of an inch, the number of degrees of torsion in the portable electrometer was 3·229 times the number of grains' weight required to balance the attractive force, and the number of degrees of torsion was 7·69 times the number of grains' weight found in other series of experiments in which the distance between the discs was thirty hundredths. According to the law of inverse squares of the distances to which the attraction between two parallel discs is subject when a constant difference of potentials is maintained between them*, the force at a distance of $\frac{1}{10}$ of an inch would have been $\frac{1}{807}$, according to the first of the preceding results, or, according to the second, $\frac{1}{854}$ of the number of degrees of torsion. The mean of these is $\frac{1}{\cdot 83}$, or 1·2, and we may consider this number as representing approximately the value in grains' weight at $\frac{1}{10}$ of an inch distance between the discs of the absolute electrometer, corresponding to one degree of torsion of the portable electrometer. By comparing the indications of the portable electrometer with those of the divided ring electrometer, and by evaluating those of the latter in terms of the electromotive force of a Daniell's battery charged in the usual manner, I find that 284 times the square root of the number of degrees of torsion in the portable electrometer is approximately the number of cells of a Daniell's battery which would produce an electromotive force (or, which is the same thing, a difference of potentials) equal to that indicated. Hence the attraction between the discs of the portable electrometer, if at $\frac{1}{10}$ of an inch distance, and maintained at a difference of potentials amounting to that produced by 284 cells, is 1·2 grain. The effect of 1000 cells would therefore be to give a force of 14·9 grains, since the force of attraction is proportional to the square of the difference of potentials between the discs. The diameter of the opposed circular areas between which the attraction observed took place, was 5·86 inches. Its area was therefore ·187 of a square foot, and therefore the amount of attraction per square foot, according to the preceding estimate for $\frac{1}{10}$ of an inch distance and 1000 cells' difference of potential, is 79·5 grains. To reduce the statement to consistent units founded on a foot as the unit of length, we may suppose, instead of $\frac{1}{10}$ of an inch, the distance between the discs to be $\frac{1}{100}$ of a foot. We conclude that, with an electromotive force or difference of potentials produced by 1000 cells of Daniell's battery, we find for the force of attraction 55·3 grains per square foot between discs separated to a distance of $\frac{1}{100}$ of a foot.

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* See § 11 of elements of mathematical theory of electricity appended to the communication following this in the 'Proceedings.'

This result differs very much from an estimate I have made according to my theoretical estimate of 2,500,000 British electromagnetic units for the electromotive force of a single element of Daniell's and Weber's comparison of electrostatic with electro-magnetic units. On the other hand, it agrees to a remarkable degree of accuracy with direct observations made for me, during my absence in Germany, by Mr. Macfarlane, in the months of June and July 1856, on the force of attraction produced by the direct application of a miniature Daniell's battery, of different numbers from 93 to 451 of elements, applied to the same absolute electrometer with its discs at ·079 of an inch asunder. These observations gave forces varying, on the whole, very closely according to the square of the number of cells used, and the mean result reduced according to this law to 1000 cells was 23·4 grains. Reducing this to the distance $\frac{1}{100}$ of a foot, and dividing by ·187, the area in decimal of a square foot, we find 54·3 grains per square foot at a distance of $\frac{1}{100}$ of a foot.

Although the experiments leading to this result were executed with great care by Mr. Macfarlane, I delayed publishing it because of the great discrepance it presented from the estimate I deduced from Weber's measurement, which was published while my preparations were in progress. I cannot doubt its general correctness now, when it is so decidedly confirmed by the electrometric experiments I have just described, which have been executed chiefly by Mr John Smith and Mr John Ferguson, working in my laboratory with much ability since the month of November. I am still unable to explain the discrepance, but it may possibly be owing to some miscalculation I have made in my deductions from Weber's result.

Glasgow College, Jan 18, 1860

Postscript, April 12, 1860

I have since found that I had inadvertently misinterpreted Weber's statement in the ratio of 2 to 1. I had always, as it appears most natural to me to do, regarded the transference of negative electricity in one direction and of positive electricity in the other direction, as identical agencies, to which in our ignorance as to the real nature of electricity we may apply indiscriminately the one expression or the other, or a combination of the two Hence I have always regarded a current of unit strength as a current in which the positive or vitreous electricity flows in one direction at the rate of a unit of electricity per unit of time, or the negative or resinous electricity in the other direction at the same rate; or (according to the infinitely improbable hypothesis of two electric fluids) the vitreous electricity flows in one direction at any rate less than a unit per second, and the resinous in the opposite direction at a rate equal to the remainder of the unit per second I have only recently remarked that Weber's expressions are not only adapted to the hypothesis of two electric fluids, but that they also reckon as a current of unit strength, what I should have called a current of strength 2, namely a flow of vitreous electricity in one direction at the rate of a unit of vitreous electricity per unit of time, and of the resinous electricity in the other direction simultaneously, at the rate of a unit of resinous electricity per unit of time.

Weber's result as to the relation between electrostatic and electro-magnetic units, when correctly interpreted, I now find would be in perfect accordance with my own results given above, if the electromotive force of a single element of the Daniell's battery used were 2,140,000 British electro-magnetic units instead of 2,500,000, as according to my thermo-dynamic estimate. This is as good an agreement as could be expected when the difficulties of the investigations, and the uncertainty which still exists as to the true measure of the electromotive force of the Daniell's element are considered. It must indeed be remarked that the electromotive force of Daniell's battery varies by two or three or more per cent. with variations of the solutions used, that it varies also very sensibly with temperature, and that it seems also to be dependent, to some extent, on circumstances not hitherto elucidated. A thorough examination of the electromotive force of Daniell's and other forms of galvanic battery, is an object of high importance, which it is to be hoped will soon be attained. Until this has been done, at least for Daniell's battery, the results of the preceding paper may be regarded as having about as much accuracy as is desirable.

I may state therefore, in conclusion, that the average electromotive force per cell of the Daniell's batteries which I have used, produces a difference of potentials* amounting to 0021 in British electrostatic measure. This statement is perfectly equivalent to the following in more familiar terms:—

One thousand cells of Daniell's battery, with its two poles connected by wires with two parallel plates of metal $\frac{1}{100}$th of a foot apart and each a square foot in area, produces an electrical attraction equal to the weight of 55 grains.

II. "Measurement of the Electromotive Force required to produce a Spark in Air between parallel metal plates at different distances." By Professor W. THOMSON, F.R.S. Received January 26, 1860.

The electrometers used in this investigation were the absolute electrometer and the portable electrometer described in my last communication to the Royal Society, and the operations were executed by the same gentlemen, Mr. Smith and Mr. Ferguson. The conductors between which the sparks passed were two unvarnished plates of a condenser, of which one was moved by a micrometer screw, giving a motion of $\frac{1}{25}$ of an inch per turn, and having its head divided into 40 equal parts of circumference The readings on the screw-head could be readily taken to tenth parts of a division, that is to say, to $\frac{1}{10,000}$ of an inch on the distance to be measured. The point from which the spark would pass in successive trials being somewhat variable and often near the edges of the discs, a thin flat piece of metal, made very slightly convex on its upper surface like an extremely flat watch glass, was laid on the lower plate. It was then found that the spark always passed between the crown of this convex piece of metal and the flat upper plate The curvature of the former was so small, that the physical circumstances of its own electrification near its crown, the opposite electrification of the opposed flat surface in the parts near the crown of the convex, and the electric pressure on or tension in the air between them could not, it was supposed, differ sensibly from those between two plane conducting surfaces at the same distance and maintained at the same difference of potentials.

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* See §§ 10, 11 of Appendix to the following communication.

The reading of the screw-head corresponding to the position of the moveable disc, was always determined electrically by making a succession of sparks pass, and approaching the moveable disc gradually by the screw until all appearance of sparks ceased. Contact was thus produced without any force of pressure between the two bodies capable of sensibly distorting their supports.

With these arrangements several series of experiments were made, in which the differences of potentials producing sparks across different thicknesses of air were measured first by the absolute electrometer, and afterwards by the portable torsion electrometer. The following Tables exhibit the results hitherto obtained.

TABLE I.—December 13, 1859. Measurements by absolute electrometer of maximum electrostatic forces* across a stratum of air of different thicknesses

Area of each plate of absolute electrometer = ·187 of a square foot.

Distance between plates of absolute electrometer = $\frac{1}{80}$ of a foot.

Length of spark in inches s	Weight in grains required to balance in absolute electrometer w	Electromotive force in units of the electrometer $\sqrt{w}$	Electrostatic force, or electromotive force per inch of air, in temporary units $\frac{\sqrt{w}}{s}$
·007	6	2·4495	349·9
·0105	9	3·0000	285·7
·0115	10	3·1622	275·0
·014	13	3·6055	257·5
·017	16	4·0000	235·3
·018	19	4·3589	242·2
·024	30	5·4772	228·2
·0295	40	6·3245	214·4
·034	50	7·0710	208·0
·0385	60	7·7459	201·2
·041	70	8·3666	204·1
·0445	80	8·9442	201·0
·048	90	9·4868	197·6
·052	100	10·0000	192·3
·055	110	10·4880	190·7
·058	120	10·9544	188·9
·060	130	11·4017	190·0

These numbers demonstrate an unexpected and a very remarkable result,—that greater electromotive force per unit length of air is required to produce a spark at short distances than at long. When it is considered that the absolute electrification of each of the opposed surfaces† depends simply on the electromotive force per unit length of the space between them, or, which is the same thing, the resultant electrostatic force in the air occupying that space, it is difficult even to conjecture an explanation. Without attempting to explain it, we are forced to recognize the fact that a thin stratum of air is stronger than a thick one against the same disruptive tension in the air, according to Faraday's view of its condition as transmitting electric force, or against the same lifting electric pressure from its bounding surfaces, according to the views of the 18th cen-

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[*] See § 3 below.

[†] See § 4 below.

tury school, as represented by Poisson. The same conclusion is established by a series of experiments with the previously-described portable torsion electrometer substituted for the absolute electrometer, leading to results shown in the following Table

TABLE II.—January 17, 1860 Measurements by portable torsion electrometer of electromotive forces producing sparks across a stratum of air of different thicknesses

Length of spark in inches s	Torsion in degrees required to balance in electrometer θ	Electromotive force in units of the electrometer $\sqrt{\theta}$	Electrostatic force, or electromotive force per inch of air, in temporary units. $\sqrt{\theta} - s$
·001	3	1·732	1732
·002	7	2·646	1323
·003	11	3·316	1105
·004	14	3·742	935
·005	18	4·243	849
·006	22	4·690	782
·007	27	5·196	742
·008	30	5·477	685
·009	33	5·744	638
·010	38	6·164	616
·011	43	6·557	596
·012	48·5	6·964	580
·013	54	7·348	565
·014	59	7·681	549
·015	66	8·124	542
·016	73	8·544	534
·017	79	8·888	523
·018	85	9·219	512

The series of experiments here tabulated stops at the distance 18 thousandths of an inch, because it was found that the force in the electrometer corresponding to longer sparks than that, was too strong to be measured with certainty by the portable electrometer, whether from the elasticity of the platinum wire, or from the rigidity of its connexion with the aluminium index being liable to fail when more than 85° or 90° of torsion were applied. So far as it goes, it agrees remarkably well with the other experiments exhibited in Table I., as is shown by the following comparative Table, in which, along with results of actual observation extracted from Table II., are placed results deduced from Table I. by interpolation for the same lengths of spark.

Table III. Experiments of December 13, 1859, and January 17, 1860, compared.

Col 1 Length of spark in inches s.	Col 2 Electromotive force per inch of air, Dec 13, in temporary units of that day $\frac{\sqrt{u}}{s}$	Col 3 Electromotive force per inch of air, Jan 17 in temporary units of that day $\frac{\sqrt{\theta}}{s}$	Col 4 Ratios of numbers in Col 3 to numbers in Col 2
·007	349·3	742	2·13
·0105	285·7	606	2·12
·0115	275·0	588	2·14
·014	257·5	549	2·14
·017	235·3	523	2·22
·018	242·2	512	2·11
			Mean 2·14

The close agreement with one another of the numbers in Col. 4, derived from series differing so much as those in Cols 2 and 3, and obtained by means of electrometers differing so much in construction, constitutes a very thorough confirmation of the remarkable result inferred above from the experiments of the first series, and shows that the law of variation of the electrostatic force in the air required to produce sparks of the different lengths, must be represented with some degree of accuracy by the numbers shown in the last column of either Table I or Table III.

The following additional series of experiments were made on precisely the same plan as those of Table II.

Table IV.—January 21, 1860. Measurements by portable torsion electrometer of electromotive forces producing sparks across a stratum of air of different thicknesses

Length of spark in inches s	Torsion in degrees required to balance in electrometer θ	Electromotive force in units of the electrometer $\sqrt{\theta}$	Electrostatic force, or electromotive force per inch of air, in temporary units $\sqrt{\theta} \div s$.
·001	3 2	1 70	1700
002	6 4	2 32	1160
003	10 5	3 24	1080
·004	13 2	3 63	907
·005	14 2	3 77	754
006	18 2	4 27	712
007	21 7	4 66	666
012	41 2	6 42	535
013	46 7	6 83	525
014	53 2	7 29	521
015	57 2	7 56	504
·016	63 2	7·95	497
·017	68 2	8 26	486
018	78 2	8 84	491

Table V.—January 23, 1860. Similar experiments repeated.

s	θ	$\sqrt{\theta}$	$\sqrt{\theta} \div s$
·001	3 5	1 87	1870
·002	6 5	2 55	1275
·003	9 5	3 08	1027
·004	12 7	3 56	890
005	15 5	3 94	788
·006	18 5	4 30	716
·007	23·0	4 80	686
·008	25 62	5·06	632
·009	30 5	5 52	613
·010	35·0	5 92	592
·011	39 5	6 28	571
·012	44 0	6 63	553
·013	50 0	7·07	544
·014	54·0	7 35	525
·015	59·0	7 68	512
·016	63 5	7 97	498
017	69 5	8 34	490
·018	74 5	8·63	479

The difference between the numbers shown in these two Tables and in Table II. above, are probably due in part to true differences in the resistance of the air to electrical disruption, but variations in the electrometer, which was by no means of perfect construction, may have sensibly influenced the results, especially as regards the differences between those shown in Table II. and those shown in Tables IV and V., which agreeing on the whole closely with one another fall considerably short of the former

TABLE VI. Summary of results reduced to absolute measure

Col 1 Length of spark in inches s	Col 2 Electrostatic forces according to simple determinations of Dec 13, 1859 $\frac{\sqrt{w}}{s}\times\frac{1}{5}\times\sqrt{\frac{32\cdot2\times8\pi^*}{187}}$ $=X$	Col 3 Electrostatic forces according to estimated average of various determinations X.	Col 4 Differences	Col 5 Pressures of electricity from either metallic surface balanced by air immediately before disruption, in grains weight per square foot † $\frac{X^2}{8\pi\times32\cdot2}$
·001		11480		162800
002		8000		70080
·003		6840		57810
·004		5820		41820
·005		5090		32010
·006		4700		27300
·007	4600	4530	+·0070	26200
008		4200		21830
·009		3900		19680
·010		3860		18300
·0105	3760	3770	−·0010	17450
·011		3720		17130
·0115	3620	3630	− 0010	16200
012		3550		15580
·013		3480		14970
·014	3390	3390	·0000	14200
·015		3320		13580
·016		3260		13110
·017	3000	3140	−·0040	11800
·018	3190	3170	+·0020	12500
·024	3100	3000		11100
·0295	2820	2820		9830
·034	2740	2740		9250
·0385	2650	2650		8650
·041	2690	2690		8900
·0445	2650	2630		8640
·048	2600	2600		8350
·052	2530	2590		7910
·055	2510	2510		7770
·058	2490	2490		7630
060	2500	2500		7720

* Distance between discs of absolute electrometer $=\frac{1}{60}$ foot $=\frac{1}{5}$ inch.

Area of each = 187 square foot.

Force of gravity at Glasgow on unit mass = 32·2 dynamical units of force, that is to say, generates in one second a velocity of 32 2 feet per second.

† This is most directly obtained by finding the force between the discs of the absolute electrometer per square foot, and reducing, according to the inverse proportion of squares of distances, to what it would have been if the distance between them had been equal to the length of the spark

In order that the different expressions, "potential," "electromotive force," "electrostatic force," "pressure of electricity from a metallic surface balanced by air," used in the preceding statement, may be perfectly understood, I add the following explanations and definitions belonging to the ordinary elements of the mathematical theory of electricity.

1 *Measurement of quantities of electricity* —The unit quantity of electricity is such a quantity, that, if collected in a point, it will repel an equal quantity collected in a point at a unit distance with a force equal to unity.

[In British absolute measurements the unit distance is one foot, and the unit force is that force which, acting on a grain of matter during a second of time, generates a velocity of one foot per second The weight of a grain at Glasgow is 32 2 of these British units of force The weight of a grain in any part of the earth's surface may be estimated with about as much accuracy as it can be without a special experiment to determine it for the particular locality, by the following expression —

In latitude λ average weight of a grain

$=32\ 088 \times (1 + \ 005133 \times \sin^2 \lambda)$ British absolute units]

2 *Electric density* —This term was introduced by Coulomb to designate the quantity of electricity per unit of area in any part of the surface of a conductor He showed how to measure it, though not in absolute measure, by his proof plane

3 *Resultant electric force at any point in an insulating fluid* —The resultant force at any point in air or other insulating fluid in the neighbourhood of an electrified body, is the force which a unit of electricity concentrated at that point would experience if it exercised no influence on the electric distributions in its neighbourhood.

4. *Relation between electric density on the surface of a conductor, and electric force at points in the air close to it.*—According to a proposition of Coulomb's, requiring, however, correction, and first correctly given by Laplace, the resultant force at any point in the air close to the surface of a conductor is perpendicular to the surface and equal to $4\pi\rho$, if ρ denotes the electric density of the surface in the neighbourhood

5. *Electric pressure from the surface of a conductor balanced by air.*—A thin metallic shell or liquid film, as for instance a soap-bubble,

if electrified, experiences a real mechanical force in a direction perpendicular to the surface outwards, equal in amount per unit of area to $2\pi\rho^2$, ρ denoting, as before, the electric density at the part of the surface considered. This force may be called either a repulsion (as according to the views of the eighteenth century school) or an attraction effected by tension of air between the surface of the conductor and the conducting boundary of the air in which it is insulated, as it would probably be considered to be by Faraday, but whatever may be the explanation of the *modus operandi* by which it is produced, it is a real mechanical force, and may be reckoned as in Col 5 of the preceding Table, in grains weight per square inch or per square foot. In the case of the soap-bubble, for instance, its effect will be to cause a slight enlargement of the bubble on electrification with either vitreous or resinous electricity, and a corresponding collapse on being perfectly discharged In every case we may regard it as constituting a deduction from the amount of air-pressure which the body experiences when unelectrified. The amount of this deduction being different in different parts according to the square of the electric density, its resultant action on the whole body disturbs its equilibrium, and constitutes in fact the resultant electric force experienced by the body.

6 *Collected formulæ of relation between electric density on the surface of a conductor, electric diminution of air-pressure upon it, and resultant force in the air close to the surface.*—Let, as before, ρ denote the first of these three elements, let p denote the second reckoned in units of force per unit of area, and let R denote the third. Then we have

$$R=4\pi\rho,$$

$$p=2\pi\rho^2=\frac{1}{8\pi}R^2.$$

7. *Electric potential*—The amount of work required to move a unit of electricity against electric repulsion from any one position to any other position, is equal to the excess of the electric potential of the first position above the electric potential of the second position

Cor. 1. The electric potential at all points close to the surface of an electrified metallic body has one value, since an electrified point, possessing so small a quantity of electricity as not sensibly to influence the electrification of the metallic surface, would, if held near the surface in any locality, experience a force perpendicular to the surface in its neighbourhood.

Cor. 2. The electric potential throughout the interior of a hollow metallic body, electrified in any way by external influence, or, if insulated, electrified either by influence or by communication of electricity to it, is constant, since there is no electric force in the interior in such circumstances

[It is easily shown by mathematical investigation, that the electric force experienced by an electric point containing an infinitely small quantity of electricity, when placed anywhere in the neighbourhood of a hollow electrified metallic shell, gradually diminishes to nothing if the electric point be moved gradually from the exterior through a small aperture in the shell into the interior Hence the one value of the potential close to the surface outside, mentioned in Cor 1, is equal to the constant value throughout the interior mentioned in Cor 2.]

8 *Interpretation of measurement by electrometer* —Every kind of electrometer consists of a cage or case containing a moveable and a fixed conductor, of which one at least is insulated and put in metallic communication, by what I shall call the principal electrode passing through an aperture in the case or cage, with the conductor whose electricity is to be tested In every properly constructed electrometer, the electric force experienced by the moveable part in a given position cannot be electrically influenced except by changing the difference of potentials between the principal electrode and the uninsulated conductor or conducting system in the electrometer. Even the best of ordinary electrometers hitherto constructed do not fulfil this condition, as the inner surface of the glass of which the whole or part of the enclosing case is generally made, is liable to become electrified, and inevitably does become so when any very high electrification is designedly or accidentally introduced, even for a very short time, the consequence of which is that the moving body will generally not return to its zero position when the principal electrode is perfectly disinsulated. Faraday long ago showed how to obviate this radical defect by coating the interior of the glass case with a fine network of tinfoil, and it seems strange that even at the present day electrometers for scientific research, as for instance for the investigation of atmospheric electricity, should be constructed with so bad and obvious a defect uncured by so simple and perfect a remedy When it is desired to leave the interior of the electrometer as much light

as possible, and to allow it to be clearly seen from any external position with as little embarrassment as possible, a cage made like a bird's cage, with an extremely fine wire on a metal frame, inside the glass shade used to protect the instrument from currents of air, &c, may be substituted with advantage for the tinfoil network lining of the glass It appears therefore that a properly constructed electrometer is an instrument for measuring, by means of the motions of a moveable conductor, the difference of potentials of two conducting systems insulated from one another, of one of which the case or cage of the apparatus forms part. It may be remarked in passing, that it is sometimes convenient in special researches to insulate the case or cage of the apparatus, and allow it to acquire a potential differing from that of the earth, and that then, as always, the subject of measurement is the difference of potentials between the principal electrode and the case or cage, while in the ordinary use of the instrument the potential of the latter is the same as that of the earth Hence we may regard the electrometer merely as an instrument for measuring differences of potential between two conducting systems mutually insulated, and the object to be aimed at in perfecting any kind of electrometer (more or less sensitive as it may be, according to the subjects of investigation for which it is to be used), is, *that accurate evaluations in absolute measure, of differences of potential, may be immediately derivable from its indications.*

9. *Relation between electrostatic force and variation of electric potential.*—§ 7, otherwise stated, is equivalent to this:—The average component electrostatic force in the straight line of air between two points in the neighbourhood of an electrified body is equal to their difference of potentials divided by their distance. In other words, the rate of variation of electric potential per unit of length in any direction, is equal to the component of the electrostatic force in that direction. Since the average electrostatic force in the line joining two points at which the values of the potential are equal, is nothing, the direction of the resultant electrostatic force at any point must be perpendicular to the equipotential surface passing through that point; or the lines of force (which are generally curves) cut the series of equipotential surfaces at right angles. The rate of variation of potential per unit of length along a line of force is therefore equal to the electrostatic force at any point.

10 ***Stratum of air between two parallel or nearly parallel plane or curved metallic surfaces maintained at different potentials*** —Let a denote the distance between the metallic surfaces on each side of the stratum of air at any part, and V the difference of potentials. It is easily shown that the resultant electrostatic force is sensibly constant through the whole distance, from the one surface to the other, and being in a direction sensibly perpendicular to each, it must (§ 9) be equal to $\frac{V}{a}$ Hence (§ 4) the electric density on each of the opposed surfaces is equal to $\frac{V}{4\pi a}$ This is Green's theory of the Leyden phial

11. *Absolute Electrometer* —As a particular case of No 10, let the discs be plane and parallel, and let the distance between them be small in comparison with their diameters, or with the distance of any part of either from any conductor differing from it in potential The electric density will be uniform over the whole of each of the opposed surfaces and equal to $\frac{V}{4\pi a}$, being positive on one and negative on the other, and in all other parts of the surface of each the electrification will be comparatively insensible Hence the force of attraction between them per unit of area (§§ 5 and 6) will be $\frac{V^2}{8\pi a^2}$, if A denote the area of either of the opposed surfaces, the whole force of attraction between them is therefore $A\frac{V^2}{8\pi a^2}$ Hence, if the observed force be equal to the weight of w grains at Glasgow, we have

$$32\ 2 \times w = A\frac{V^2}{8\pi a^2},$$

and therefore

$$V = a\sqrt{\frac{32\ 2 \times 8\pi \times w}{A}}$$

Addition, dated April 12, 1860

Experiments on precisely the same plan as those of Table I. December 13, have been repeated by the same two experimenters, with different distances from 3 to 6 of an inch between the plates of the absolute electrometer, and results have been obtained confirming the general character of those shown in the preceding Tables

The absolute evaluations derived from these later series, must be more accurate than those deduced above from the single series of December 13, when the distance between the plates in the absolute electrometer was only 2 of an inch I therefore by permission add the following Table of absolute determinations —

Length of spark in inches *s*	Electrostatic forces according to estimated average of determinations of February 15, 23, 28, and 29, and March 2 X
0034	5793
005	5574
·006	5688
·0075	4862
·0111	4351
·0161	3287
·0222	3125
·023	3029
·0271	3055
·0356	2925
·0416	2865
·0522	2841

These results, as well as those shown in the preceding Tables, demonstrate a much less rapid variation with distance, of the electrostatic force preceding a spark, at the greater than at the smaller distances. It seems most probable that at still greater distances the electrostatic force will be found to be sensibly constant, as it was certainly expected to be at all distances. The limiting value to which the results shown in the last Table seem to point must be something not much less than 2800. This corresponds to a pressure of 9600 grains weight per square foot We may therefore conclude that the ordinary atmospheric pressure of 14,798,000 grains per square foot, is electrically relieved by the subtraction of 9600 on two very slightly convex metallic surfaces, at a distance of $\frac{1}{50}$th of an inch or more, before the air between them is cracked and a spark passes. By taking into account the result of my preceding communication to the Royal Society, we may also conclude that a Daniell's battery of 5510 elements can produce a spark between two slightly convex metallic surfaces at $\frac{1}{50}$th of an inch asunder in ordinary atmospheric air.

III "On the Lines of the Solar Spectrum" By Sir David Brewster, K H., D C L., F.R.S., and Dr. J. H. Gladstone, F.R S

Received January 26, 1860

(Abstract)

In a paper in the Transactions of the Royal Society of Edinburgh for 1833, Sir David Brewster stated that he had examined the lines of the solar spectrum, and those produced by the intervention of nitrous acid gas, and had delineated them on a scale four times greater, and in some parts twelve times greater than that employed in the beautiful map of Fraunhofer None of these drawings, however, were published at the time, they were increased by frequent observations continued through succeeding years, and now having been collated, arranged, and added to by Dr. Gladstone, they form the diagrams accompanying this paper

The figures consist of—

1st A map of the whole spectrum 58 inches long, and exhibiting about 1000 lines and bands. This map includes a great prolongation of the spectrum at the least refrangible end, before A, with a series of bands and lines not hitherto described

2nd, 3rd, 4th, and 5th. Enlarged delineations of the portions of the spectrum between A and B, and between E and F, exhibiting additional lines, with still more magnified views of the groups *a* and *b*

6th. A map of the two extremities of the solar spectrum as observed by Dr. Gladstone about noon-day at midsummer, consequently when the sun was at its greatest altitude This shows several bands between A and B, and a series of lines in the lavender rays extending as far as M Becquerel's N, and corresponding evidently with the maps published by him and by Professor Stokes, with the addition of finer lines. Yet this map represents the extreme spaces of the spectrum where there is no effect on the organ of vision, while that of M. Becquerel represents the want of chemical action, and that of Professor Stokes the absence of fluorescent power.

7th. A map of the "atmospheric lines" compiled from the independent observations of the two authors. These lines and bands are visible only when the sun is rising or setting, that is to say, when his beams traverse a long stratum of our atmosphere. In some cases

they are merely the deepening of bands seen at any time, in other cases they are bands which appear for the first time when the sun is close to the horizon Professor Piazzi Smyth has represented some of these lines in his delineations of the spectrum as observed from the Peak of Teneriffe, whence he had the advantage of seeing the sun at an altitude of —1° 1. The most remarkable of the atmospheric lines are situated in the orange and yellow spaces, and one band just beyond D is discernible in the diffused light of a dull day at any hour, though it covers what is about the brightest part of the prismatic image obtained from direct sunshine The western sky after sunset exhibits these phenomena in a striking manner, and with some variations that do not appear to depend altogether on the absence or presence of moisture, although when the sun looms red through a fog these lines also make their appearance They are in no respect due to the mere reduction in the quantity of light

The dispersion and absorption of the more refrangible rays by the atmosphere, and by fogs, smoke, and such media as dilute milk and water, is a quite independent phenomenon

8th and 9th Enlarged views of A and B, when the light is acted upon by a long passage through the atmosphere

10th A map of the spectrum, exhibiting on a large scale the dark lines and bands which were seen by Sir David Brewster when nitrous acid gas is interposed between the prism and the source of light. Their position is identified by the insertion of the principal lines of the solar spectrum. They differ considerably from a smaller drawing of the same by Professor W. A. Miller, who employed a deeper stratum of the red gas.

The light of the moon, which is only that of the sun reflected from her surface, exhibits all the principal lines from about B to H, and no fresh ones, and when the luminary was near the horizon, the more prominent atmospheric lines were detected. The green colour was observed to extend a little beyond F in the spectrum of moonlight, and the space between G and H appeared lavender or lavender-grey instead of violet

In respect to the origin of these lines, it is conceivable that the light when emitted from the photosphere itself is deficient in these rays, or that they are due to absorption by the atmosphere of the sun, or by that of the earth. The first of these suppositions scarcely admits of a positive proof. If the second be true, it might be expected that the light from the edge of the solar disk would exhibit more of these absorption bands than that from the centre, which must have traversed a smaller amount of atmosphere, but such was not found to be the case The third supposition is favoured by the fact that the atmosphere has unquestionably much to do with the manifestation of many of these lines, and by the analogy of the bands produced by nitrous acid gas, bromine vapour, and other absorbent media The *experimentum crucis* of observing an artificial light through a long space of air was attempted by means of the revolving light on Beachy Head, as seen from Worthing at a distance of twenty-seven miles It gave a negative result, but on account of the great difficulty of detecting slight breaks in a faint thread of light, no great reliance is to be placed on the experiment A similar doubt rests on the authors' observations of fixed stars, and on the non-recognition by Fraunhofer of the ordinary lines in the light of Sirius and Castor, while on the other hand he did detect D and *b* in that of other stars The origin of these lines is still an open question

The spectra of artificial flames sometimes exhibit bright lines coincident with the dark spaces of the solar spectrum. Thus the yellow band in the flames of soda, and several other substances, is identical in refrangibility with D, but the most remarkable case is that of charcoal or sulphur burnt in nitre, the spectrum shows three very prominent lines, two of which coincide with A and D, while a faint red line appears at B, and a group between it and A

A map is also given of the bright lines, principally orange, that make their appearance when nitrate of strontia is placed on the ignited wick of a spirit-lamp.

IV. "On some New Volatile Alkaloids given off during Putrefaction." By F. CRACE CALVERT, Ph.D., F.R S., &c. Received February 23, 1860.

Some eighteen months ago my friend Mr. J A Ransome, surgeon to the Royal Infirmary, Manchester, induced me to make some researches with the view of ascertaining the nature of the products given off from putrid wounds, and more especially in the hope of throwing some light upon the contagion known as hospital gangrene I fitted up some apparatus to condense the noxious products from such wounds, but the quantity obtained was so small, that it was necessary for me to acquire a more general knowledge of the various substances produced during the putrefaction of animal matter, before I could determine the nature of the products from sloughing wounds I therefore began a series of experiments, the general results of which I now wish to lay before the Society

Into each of a number of small barrels twenty lbs of meat and fish were introduced, and to prevent the clotting together of the mass, it was mixed layer by layer with *pumice-stone* The top of each barrel was perforated in two places, one hole being for the purpose of admitting air, whilst through the other a tube was passed which reached to the bottom of the barrel. This tube was put in connexion with two bottles containing chloride of platinum, and these in their turn connected with an aspirator. By this arrangement air was made to circulate through the casks, so as to become charged with the products of putrefaction and to convey them to the platinum salt. A yellow amorphous precipitate soon appeared, which was collected, washed with water and alcohol, and dried This precipitate was found to contain C, H, and N, but what is highly remarkable, sulphur and phosphorus enter into its composition The presence of C, H, and N was ascertained by elementary analysis, for the sulphur and phosphorus, a given weight of the platinum salt, 0 547 *grm*, was oxidized with nitric acid, and gave 0 458 grm of sulphate of baryta =11 per cent. of sulphur, and 0·266 of pyrophosphate of magnesia =6 01 per cent of phosphorus. I also ascertained the presence of these two substances by heating a certain quantity of the platinum salt with strong caustic ley, when a liquid, volatile and inflammable alkaloid was obtained, whilst the sulphur* and phosphorus remained combined with the alkali and were easily detected I satisfied myself during these researches, which have lasted more than twelve months, that no sulphuretted nor phosphuretted hydrogen was given off, and my researches, as far as they have proceeded, tend to prove that the noxious vapours given off during putrefaction, contain the N, S, and Ph of the animal substance, and that these elements are not liberated in the simple form of ammonia, and sulphuretted and phosphuretted hydrogen. I also remarked during this investigation, that, as putrefaction proceeds, different volatile bodies are given off.

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* Some of the platinum salt was treated with C S_2, which did not remove any free S, and the beautiful orange-yellow colour of the precipitate showed the absence of sulphuret of platinum

Before concluding, I may add, that when the platinum salts are heated in small test-tubes, they give off vapours, some acid and some alkaline, possessing a most obnoxious and sickening odour, very like the odours of putrefaction, and that at the same time a white crystalline sublimate, which is not chloride of ammonium, is formed.

As I foresee that these researches will occupy several years, I have deemed it my duty in the mean time to lay the above facts before the Society.

March 1, 1860

Sir BENJAMIN C BRODIE, Bart., President, in the Chair

In accordance with the Statutes, the Secretary read the following list of Candidates for election into the Society —

Frederick Augustus Abel, Esq.
Somerville Scott Alison, M D.
Alexander Armstrong, M D.
Thomas Baring, Esq.
Charles Spence Bate, Esq.
John Frederic Bateman, Esq
Henry Foster Baxter, Esq
William Brinton, M.D
Edward Brown-Séquard, Esq
Thomas William Burr, Esq.
Richard Christopher Carrington, Esq
Alexander Ross Clarke, Capt. R.E.
William White Cooper, Esq
Joseph Cubitt, Esq
Henry Duncan Preston Cunningham, Esq , R N.
Thomas Rowe Edmonds, Esq
James Fergusson, Esq.
Francis Galton, Esq
Joseph Henry Gilbert, Ph.D.
Robert Philips Greg, Esq
John Braxton Hicks, M.D.
Sir William Jardine, Bart
Thomas Hewitt Key, M.A.
Waller Augustus Lewis, Esq
Joseph Lister, Esq.
Edward Joseph Lowe, Esq.
David Macloughlin, M.D.

Rev. Robert Main, M A
Gavin Milroy, M D.
Rev. Walter Mitchell.
Ferdinand Mueller, M D
Robert William Mylne, Esq
William Newmarch, Esq
Andrew Noble, Capt R.A
Roundell Palmer, Esq., Q C
Edmund Alexander Parkes, M.D.
George Peacock, Esq.
John Thomas Quekett, Esq
Rev Thomas Robinson, D D
Maxwell Simpson, Esq.
Edward Smith, M D
Sir James Emerson Tennent
Henry Ward, Capt R E
J Forbes Watson, M D
C Greville Williams, Esq
Frederick Marow Eardley Wilmot, Lieut.-Col R A
Matthew Digby Wyatt, Esq

The following communications were read —

1 "On the Electrical Phenomena which accompany Muscular Contraction" By Professor C. Matteucci. Communicated by Dr. Sharpey, Sec R.S Received January 7, 1860

Dr Radcliffe has recently communicated to the Royal Society some observations on the nature of the electrical phenomena accompanying muscular contraction It is known that M du Bois-Reymond admits that the muscular current diminishes during contraction, and that he attributes the phenomena indicated by the galvanometer to the momentary predominance of currents due to the polarization of the electrodes of platinum over the muscular current In my last memoir on Electro-physiology, which was communicated to the Royal Society and appeared in the Philosophical Transactions for 1856, I proved that these phenomena take place independently of the existence of secondary currents of the electrodes, and I hence concluded, at least as regards the muscles of frogs, that during contraction there is a current, or rather an instantaneous electrical discharge, which takes a contrary direction to that of the *relaxed gastrocnemius*, and in general to that of the current which is found on applying the extremities of the galvanometer to the extremities of the limbs of a frog.

In order to avoid the influence of secondary polarity, M. du Bois-Reymond, and after him several other German physiologists, have thought it expedient to contract and tetanize the gastrocnemius before closing the circuit of the galvanometer, the deviation thus obtained is feebler than that which is due to the current of a muscle in repose, but never in a contrary direction to that due to this current

I have already remarked* that this result accords with that which is obtained by the ordinary experiment, in which the muscular current is in circulation previously to the contraction of the muscle In fact, we know that by continuing to keep the muscle in contraction, above all when the muscle remains tetanized, the electric phenomenon accompanying contraction (the effect of which is to produce a deviation of the needle in a contrary direction to that of the current of a relaxed muscle) becomes gradually less intense as the contractions are more and more feeble.

The method employed by Dr. Radcliffe is the same as that which I followed in my latest experiments, that is, he made use of amalgamized plates of pure zinc as electrodes, immersed in a neutral solution of sulphate of zinc, and after having ascertained that there was nothing to fear from the effects of secondary polarity, he says that he finds that the needle deviated by the muscular current descends, during contraction, towards zero, but *only more slowly* than it would have done had the circuit been opened

Dr Radcliffe next examines another of my experiments, in which, instead of placing a *gastrocnemius* in the circuit, I employ a thigh cut transversely at the upper extremity, so that the needle remains deviated in a contrary direction to that of the gastrocnemius In this arrangement of the experiment, when contraction is produced, the deviation of the needle increases, which is perfectly in accordance with the idea that during contraction a muscular current is developed in a contrary direction to the current of the relaxed gastrocnemius. Dr. Radcliffe attempts to explain this result by supposing (if I rightly understand his idea) that during contraction the contacts with the electrodes are deranged so as to facilitate the passage of the current of the relaxed muscle

Being unwilling to remain in doubt as to the nature of the electrical phenomena of muscular contraction, I have of late repeated and varied my experiments.

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* Nuovo Cimento, September 1858, p 238

As to Dr. Radcliffe's first remark, I shall only observe that in the principal experiment the needle does not merely move slowly towards zero during contraction, but is seen, during the first contractions, especially when the frog operated on is vivacious, to move rapidly down to zero, to oscillate, to pass to the opposite side, and sometimes even to remain fixed, while thus deviated, for a very short interval of time This result, which is easily obtained and can be verified without difficulty, is the same, whether the electrodes are of platinum, like those employed by M du Bois-Reymond, or of zinc.

It is easy to understand that, in order to succeed in these experiments, it is desirable that the needle should be as little deviated as possible before the contractions: this object is best ensured in the following way —I prepare the frog by reducing it to two thighs, leaving a single lumbar nerve in order to obtain contractions in one of the thighs. Instead of saturated solution of sulphate of zinc, I employed a weak solution of this salt, in order to avoid any alteration of the surface of the muscles, and finally, in order to maintain exactly the same points of contact between the two electrodes and the two near points of the middle portion of the thigh, I employ two fine woollen cords or two thin strips of card-board fixed with sealing-wax on a plate of glass and soaked in the same solution The experiment is made by applying the glass plate with a certain pressure on the thigh, so that the two cords on one side touch the thigh, and on the other are placed in contact with the cushions of flannel or card-board which are immersed together with the electrodes, according to the method followed by M du Bois-Reymond.

I think it useful to describe in a few words a little apparatus which affords a good deal of facility for making these experiments. It consists in a small square block of wood, with a cavity deep enough to receive the electrodes and the cushions It is hardly necessary to say that this cavity is coated with a varnish of sealing-wax and divided in the middle by a glass plate. Another cavity in the same block serves as a recipient for the two thighs; the sciatic nerve extends beyond the block, and rests on two platinum wires which communicate with the pile or with the electro-magnetic machine. The communication between the thigh and the electrodes is established by means of the glass plate in the manner above described, that is, I press this strip of glass slightly on the middle of the thigh on one side, and at the same time the extremities of the two woollen *cords come to rest on the cushions* The movements of the needle are observed through a telescope (lunette) I have repeated this experiment thirty or forty times. Sometimes, and this case is the most frequent, the first deviation produced by the muscle in repose is directed in the same sense as that of the current of the gastrocnemius, sometimes the current is null, or almost null, sometimes, and this case is the most rare, the deviation is in a contrary direction, and this occurs most frequently in operating on the hinder portion of the thigh

In all these experiments, the moment that the thigh begins to contract, the needle moves in a constant direction, the deviation which intervenes is greater or less according to the force of the contraction, and indicates constantly a descending discharge or current of extremely short duration, which traverses the thigh in the direction of the ramification of the nerves, and in a contrary direction to the current of the gastrocnemius

II. "An Inquiry into the Muscular Movements resulting from the action of a Galvanic Current upon Nerve" By Charles Bland Radcliffe, M D, F R.C.P, Physician to the Westminster Hospital. Communicated by Dr. Sharpey, Sec R.S. Received February 2, 1860

(Abstract)

In a lecture delivered about two years ago*, in which he treats among other things of the muscular movements resulting from the action of a galvanic current upon a motor or mixed nerve, Professor Claude Bernard says that some of the more important of these movements have been overlooked, and he quotes an account of some investigations by Dr. Rousseau of Vesy, which do away with certain very perplexing variations in the order of these movements

The movements resulting from the action of a galvanic current upon nerve are usually divided into the three periods of double, alternate, and single contraction which are set down in the following Table :—

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* Leçons sur la Physiologie et Pathologie du système nerveux. Tome 1 Leçon 10. Paris, 1858

TABLE I.—*The Nerve divided and lifted up at its end*

	Direct Current		Inverse Current	
	Beginning	End	Beginning	End
Period of double contraction	Strong contraction	Contraction	Contraction	Contraction
Period of alternate contraction	Contraction	0	0	Contraction
Period of single contraction	Contraction	0	0	0
	Apparent irregularity—" Voltaic alternatives "			

Professor Bernard proposes to place another period before the first of these—a period corresponding to the normal unexhausted and undisturbed state of nerve, and characterized by contraction at the beginning of the two currents, direct as well as inverse

The investigations of Dr Rousseau show how it is that the order of these contractions is altered by certain changes in the arrangement of the nerve acted upon by the current If *the nerve acted upon be divided and lifted up at its end*, so as to break the circuit of the *derived current**, the order of contraction is that which is put down in the preceding Table , if *the nerve acted upon be raised in a loop* (either without dividing it, or, after dividing it, by dropping down the end), so as not to break the circuit of the derived current, the order of contraction is that which is represented in the following Table :—

TABLE II —*The Nerve in a loop*

	Direct Current		Inverse Current.	
	Beginning	End.	Beginning	End.
Period of double contraction	Strong contraction	Contraction	Strong contraction	Contraction
	Contraction	Contraction.	Strong contraction	Contraction
Period of alternate contraction .	0	Contraction	Contraction	0
Period of single contraction .	0	0	Contraction	0
.	Apparent irregularities—" Voltaic alternatives."			

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* Figures 3 & 4 on page 354 may serve to illustrate all that need be said respecting *the derived current* In figure 3, the sciatic nerve of a frog's leg is

Now Dr. Rousseau shows that the order of contraction set down in the second Table is due to the action of this *derived current*. He shows, also, that by excluding the derived current (which he does by means of an ingenious triple arrangement of poles called the "rhéophore bifurqué"), the order of contraction becomes one and the same in the case where the nerve is divided and lifted up at its end, and in the case where the nerve is arranged in a loop, the order being that which is set down in the first Table.

On inquiring into these matters experimentally, the author finds reason to believe that Professor Bernard has wandered into some degree of error, and that the path of inquiry opened out by Dr. Rousseau is not only a right path, so far as its discoverer has traced it out, but that it leads to the explanation of some very curious alternating movements which have not hitherto been described. He has been led, also, to form certain conjectures respecting the *modus operandi* of electricity in muscular motion which he trusts may serve to simplify this very difficult and complex subject.

1. When nerve is in its normal, unexhausted, undivided state, there is, as Professor Bernard points out, contraction at the beginning of both currents, inverse as well as direct, and at the beginning only, *if a feeble current be used*. This, for example, will be the result of the application of the feeble current produced by partially moistening the small galvanic forceps of Bernard with saliva. But it is also a fact, that a stronger current—the current for example of a Pulvermacher's chain of ordinary length moistened with vinegar—will produce contraction at the end as well as at the beginning of both currents (as in the period of double contraction), if applied to a nerve similarly circumstanced. Nay, it is a fact, that the feeble current of the forceps will give contraction at the beginning of both

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arranged *in a loop* across the poles P N of a galvanic apparatus; the *primitive current*, indicated by the black arrow, passes by the shortest route from the positive pole P to the negative pole N; the *derived current*, indicated by the dotted arrows, passes by the longest route between these poles, and as it also proceeds from the positive pole P to the negative pole N, it passes in the contrary direction to that of the primitive current. In fig. 4, the sciatic nerve is divided between P and the thigh, as is meant where the nerve is spoken of as *divided and lifted up at its end*, and being divided, the only current passing is the primitive current, for the circuit of the derived current being broken, there can be no derived current in this case.

currents, and at this time only, *after* the stronger current of the chain has produced contractions at the end as well as at the beginning of both currents, and that we may produce alternately again and again the contraction confined to the beginning, and a contraction occurring at the end as well as at the beginning, of the currents, by applying alternately the feeble current of the forceps and the stronger current of the chain The author finds, also, that the feeble current of the forceps will produce contraction at its end as well as at its beginning, if the nerve be raised and placed as a loop across the points of the forceps, and not only so, but that the same current will produce contraction only at its beginning, if it be applied *after slipping away the points of the forceps*, and so allowing the nerve to fall back upon the muscles Hence the single contraction at the beginning of a *feeble* inverse or direct current, and not at the end, instead of indicating, as Professor Bernard supposes, the normal state of undisturbed and unexhausted irritability in the nerve, must only be looked upon as the result of the action of a feeble current under particular circumstances In a word, the fact is one which reflects the strength of the current rather than the condition of the nerve.

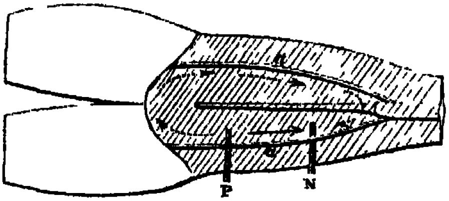
2 The curious alternating movements, which do not appear to have been described hitherto, and which may be explained by means of the key which Dr. Rousseau has put into our hands, are best seen when the current is made to act upon the lumbar nerves of one side, but they are also seen in the case where a loop of sciatic nerve is acted upon.

Take the back, loins, and hind limbs of a frog with the lumbar nerves properly exposed, raise the nerves on one side into a loop without dividing them, place them over the platinum poles of a galvanic apparatus (a Pulvermacher's chain of ordinary length), and pass the current. On doing this, as might be expected, there is in the first instance, contraction in the limb to which the nerves acted upon belong, *but this contraction is slight and transient when compared with the contraction which is set up in the opposite limb, the nerves of which are not acted upon* In this opposite limb, indeed, the contraction is sure to be both strong and tetanic. A little later (and it is to the phenomena of this stage that the author wishes to direct attention), and the results are as follows —With the *inverse* *primitive current* there is contraction in the leg belonging to the same side when the current begins to pass, and contraction in the leg belonging to the opposite side when it ceases to pass, with the *direct primitive current* this order of contraction in the two legs is reversed.

In bringing about these curious alternations, the action of a *derived current* is obviously concerned, for on excluding this current by means of Dr. Rousseau's rhéophore bifurqué, they come to an end, and the movements resulting from the action of the current are confined to the leg, the nerves of which are directly acted upon It is evident, also, that a derived current is what is wanting to produce the contraction in the limb belonging to the opposite side, for after breaking the circuit of the derived current by dividing the lumbar nerves where they emerge from the spine, and separating the divided ends, and after then completing the circuit by dropping down the end of the divided nerve, or by bridging over the gap by a piece of wet string or paper, by a strip of the animal's skin, by a piece of wire, or by any other conductor, it matters not what, the contractions occur alternately in the two legs just as they did before the nerve was divided Nay, it may be argued from the following experiment, that reflex nervous action has nothing to do in producing these alternations. Divide the lumbar nerves on one side, not where they emerge from the spine, but where they pass into the thigh, raise the divided end of the nerve, and place it across the poles of the galvanic apparatus In this case the circuit of the derived current is broken, and the action of this current is therefore put out of the question In this case, the nerve acted upon by the current is still in connexion with the spinal cord, and through the cord and the nerves proceeding from this cord, with the limb on the opposite side, and hence it might be supposed that the current might irritate the cord, and so provoke contraction in the limb on the opposite side. But the simple fact is, that the current may be passed inversely or directly without producing contraction anywhere, except now and then a few flickers in the muscular fibres in the lumbar region of the side corresponding to that of the nerve operated upon The simple fact, indeed, appears to show that reflex nervous action can have nothing to do with the contractions in the limb belonging to the opposite side, which contractions are produced by the action of the galvanic current on one set of lumbar nerves, and, certainly, with the key furnished by Dr. Rousseau, reflex nervous action is not required to explain the phenomenon.

The following diagram will give the case in which the lumbar nerves on one side are acted upon by the *inverse primitive current*, *a a* being the lumbar nerves, P N the poles of the galvanic apparatus, the black arrow the primitive current, the dotted arrows the derived current. The results, as seen in contraction in the limb belonging to the same side, or in that belonging to the opposite side, are seen in the Table below the figure The case is plain

Fig 1

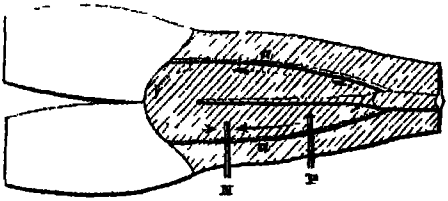


	The Inverse Current	
	Beginning	End
On the same side	Contraction	0
On the opposite side	0	Contraction

On the side acted upon by the inverse primitive current, the portion of nerve nearest to the muscles supplied by the nerve (the muscles of the leg) is traversed, not by the inverse primitive current, but by a *direct* derived current, and hence we should expect to find in the leg on this side (for at the time of these alternate contractions the nerve is in the state in which the current produces contraction alternately at the beginning of the direct current and at the end of the inverse current) the effects of a direct current—contraction at the beginning of the current We should expect to find this; for of two currents acting upon the same nerve, it is the one nearest to the muscles supplied by the nerve which acts upon these muscles In the limb on the opposite side we should expect, on the contrary, the effects of an inverse current—contraction at the end of the current, for the lumbar nerves on this side are traversed by an *inverse* derived current; and this, as the Table shows, is actually the case

A similar diagram and table will show that the results of passing a *direct primitive current* through a portion of the lumbar nerves on one side are in accordance with the same law. In this case, as in the other, the acting current on both sides is the derived current On the side acted upon by the direct primitive current, the acting derived current (acting because nearest to the muscles supplied by the nerve) is *inverse*, and therefore the limb on this side ought to contract at the end of the current On the opposite side, the course of the derived current is *direct*, and therefore the limb on that side ought to contract when the current begins to pass and so it is

Fig 2

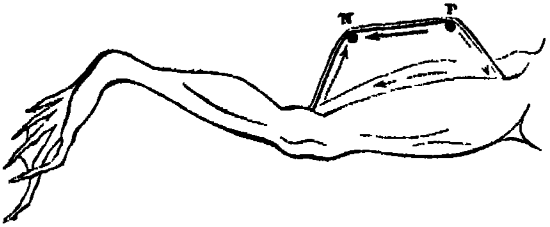


	The Direct Current.	
	Beginning	End
On the same side	0	Contraction
On the opposite side	Contraction	0

The results of the action of a galvanic current upon a loop of sciatic nerve are, after a time, analogous to those which have just been mentioned At first, the contraction attending upon the beginning and ending of both currents affects the whole limb, after a time, the leg and thigh contract alternately, in an order which changes with the direction of the current

Let the following diagram and table represent the case in which a loop of sciatic nerve is acted upon by the *direct primitive current*, *a* being the nerve, P N the poles of the galvanic apparatus, the black arrow the primitive current, the dotted arrows the derived current, and it will be seen that the portion of nerve between the negative pole and the leg is acted upon by an *inverse* derived current, and that the thigh is traversed by a *direct* derived current Thus—

Fig 3

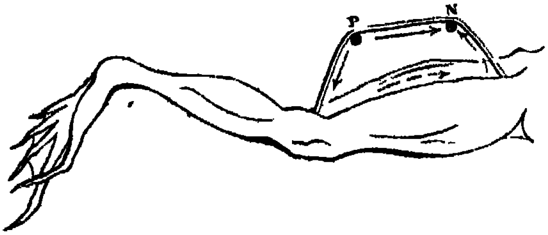


	The Direct Current	
	Beginning	End.
Thigh	Contraction	0
Leg	0	Contraction

Hence there ought to be, as there is in fact, and as the Table shows, contraction in the thigh when the current begins to pass, and in the leg when the current ceases to pass

A similar diagram and table will give the case in which a loop of sciatic nerve is acted upon by the *inverse primitive current*, and show at a glance that the leg ought to contract at the beginning of the current, because the current, acting upon the portion of nerve nearest

Fig 4



	The Inverse Current	
	Beginning	End
Thigh	0	0
Leg	Contraction	0

to the leg, is *direct* derived current. The diagram will also seem to show that the thigh ought to contract at the end of the current, for the thigh is traversed by *inverse* derived current In fact, however, the thigh does not contract either at the beginning or at the end of the current, and this perhaps is not to be wondered at, for the author finds that contraction attends upon the beginning of a *direct* current of a given strength for some time after it has ceased to attend upon the end of an *inverse* current of the same strength.

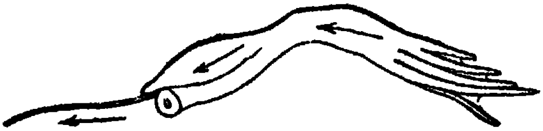
3 The *modus operandi* of galvanism upon a motor or mixed nerve is a subject beset with difficulties, but some of these difficulties do not appear to be altogether insurmountable.

(*a*) In looking at the movements belonging to the first period—*that of double contraction* (*vide* Table I)—it is not difficult to find a reason which will in some degree explain how it is that contraction is confined to the beginning and end of the current It is not difficult to see that the beginning and ending of the galvanic current in the nerve may involve certain changes in the strength of the nerve-current, and that these changes may in their turn give rise to momentary induced currents in the nerve and in the neighbourhood, for such momentary currents are induced, not only when a current begins to pass and when it ceases to pass, but also at the moments when it undergoes any change of strength. It is not difficult to see, also, that the muscular fibres to which the nerve is distributed may be the seat of some of the secondary currents thus induced, and that these fibres may be thrown into contraction by these currents. Nor is it difficult to see—if the contraction be thus connected with the induced currents—that there will be no contraction in the interval between the beginning and ending of the inducing galvanic current, for if the inducing current exhibits no variation in strength, there is no secondary current induced in this interval.

(*b*) It is, perhaps, too much to expect at present a full explanation of *the second period of contraction*—of that period, that is, in which the contraction alternates at the beginning of the direct and at the end of the inverse current, but the author is disposed to think that a partial answer may be found in the collation of the three facts which follow.

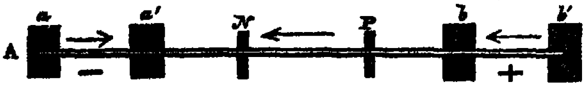
The first fact is this—that the direction of the nerve-current in the sciatic and lumbar nerves of a frog (except in those last moments of all in which the action of the galvanic current upon the nerve gives rise to the "voltaic alternatives") is *inverse*. In these last moments the nerve-current in these nerves may be sometimes direct, sometimes inverse, and this change of direction may take place more than once, but except in these last moments, the author finds the direction of the nerve-current to be invariably inverse

Fig 5



The second fact is furnished by Professor du Bois-Reymond in an experiment in which the two ends of a long portion of nerve are placed upon the cushions of two galvanometers, and the middle of the nerve is laid across the poles of a galvanic apparatus Looking at the needles of the galvanometers before passing the galvanic current, they are seen to diverge under the action of the nerve-current, and from the direction of the divergence, it is evident that this current passes from the end to the side of the nerve. Looking at the needles while the galvanic current is passing, one needle is found to move still further from zero, the other is found to return towards zero. Let A B be the nerve, let the arrows *a a'* and *b b'* be the nerve-currents included between the cushions *a a'* and *b b'* of two galvanometers, and let the arrow P N be the current between the poles P N of the galvanic apparatus, and under this arrangement the needle of the galvanometer will recede, and show *increase* of current (+) at the end B, where the nerve-current and galvanic current coincide in their direction, and at the end A, where the two currents, natural and artificial, do not coincide in their direction, the needle of the galvanometer will go back, and show decrease of nerve-current (—).

Fig 6.



The third fact, which has been recently furnished by Professor

Eckardt, is to be found in an experiment which may be illustrated by means of the two following figures In this experiment, the nerve of a properly prepared frog's leg is placed, one portion (that nearest to the leg) across the poles I I′ of an induction coil, another portion across the poles P N of a galvanic apparatus Having done this, the leg is first thrown into a state of tetanus by passing a series of induced currents, and then (the tetanizing influence still continuing in operation) the continuous current of the galvanic apparatus is transmitted from P to N This is the experiment The result is that the tetanus ceases when, as in fig 7, the inverse current passes, and continues when, as in fig 8, the direct current passes. Nor is this result altered by inverting the order in which the continuous and induced currents are made to act upon the nerve Thus the induced currents produce contraction if applied after the direct continuous current, but not if applied after the inverse continuous current Nay, it would even seem as if the direct current is actually favourable to contraction, for a solution of salt, which of itself is too weak to produce tetanus when applied to a nerve, will have this effect when a direct current is made to pass through another portion of the same nerve In performing this experiment, Professor Eckardt proceeds as follows:—First of all he tetanizes a frog's hinder limb by placing a portion of the nerve nearest to it in a strong solution of salt, after this he adds water until the strength of the saline solution is no longer sufficient to keep up a state of contraction in the muscles, then, all things being as they were, he passes the direct current through a portion of nerve which is not immersed in the solution *The result is that the tetanus immediately returns.*

Fig 7

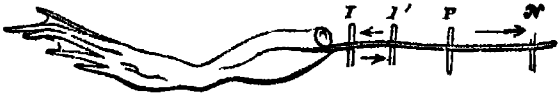
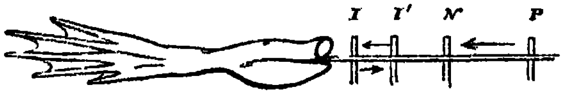


Fig 8



Now, on comparing this last fact with the two previous facts, we may have, as it seems to the author, some insight into the mode in which the galvanic current acts upon nerve in the period of alternate contraction. On the one hand, it is seen that tetanus is prevented or arrested by the inverse current, that is to say, tetanus is prevented or arrested when (as the first and second facts show) the galvanic current coincides in direction with, and imparts power to, the nerve-current. On the other hand, it is seen that tetanus is *not* prevented or arrested by the direct current; that is to say, tetanus is *not* prevented or arrested when (as the first and second facts still show) the galvanic current differs in direction from, and diminishes the power of, the nerve-current The one result is in harmony with the other, for if contraction is counteracted by imparting power to the nerve-current, it is to be expected that contraction will be favoured by detracting power from the nerve-current, and certainly it is no matter of wonder that contraction should be favoured by detracting power from the nerve-current, for it is an established fact that *rigor mortis* is coincident with absolute extinction of the nerve and muscular currents, and that ordinary contraction is attended by unmistakeable *weakening* of these currents. It is also an established fact, that muscular contraction is produced by the *discharge* of ordinary statical electricity, and not by the charging and charge Nay, it is not improbable that the contractions at the beginning and ending of the current, in the period of double contraction, which contractions have been referred by the author to the action of induced currents, may in reality be due to the *withdrawal* rather than to the *communication* of these currents; for these induced currents are of momentary duration, disappearing at the very instant of appearing, and exhibiting peculiarities in disappearing which connect the disappearance with the *discharge* of statical electricity, rather than with the more quiet cessation of current electricity.

And if this be so—if the inverse current antagonises and the direct current favours contraction—then we may in some degree understand how it is that contraction occurs alternately at the beginning of the direct, and at the end of the inverse current.

When the *inverse current* passes, there is no contraction at the beginning of the current, for the influence of this current upon the nerve-current is one which antagonizes contraction, when the inverse current ceases to pass there is contraction, for then the influence which antagonized contraction is removed When, on the other hand, *the direct current* passes, there is contraction at the beginning of the current, for the influence of the current upon the nerve-current is one which favours contraction, when the direct current ceases to pass, there is no contraction, for then the influence is no longer one which favours contraction.

(*c*) In the third period—*that of single contraction*—the muscular movements resulting from the action of a galvanic current upon nerve are at first sight somewhat perplexing, but with a little thought, it may be seen that the same key will apply to their interpretation

If, as has just been mentioned, contraction attends upon the beginning of the direct current because this current is found to favour contraction, it is not difficult to find a reason which will explain in some degree, not only why in *the period of double contraction* the strongest contraction is at the beginning of the direct current, but also why in the first part of the period now under consideration—*that of single contraction*—contraction should continue to attend upon the *direct* current after it has ceased to attend upon the *inverse* current. Nor are the apparent irregularities in contraction, the "voltaic alternatives," entirely inexplicable, for it may be that these seeming irregularities—this apparent shifting of contraction from the beginning of the direct to the beginning of the inverse current, and so backwards and forwards once and again—may be nothing more than the natural consequence of the changes which at this time have taken place, and are taking place, in the direction of the nerve-current

III. "Letter from Lord HOWARD DE WALDEN AND SEAFORD, Her Majesty's Minister at Brussels, to Lord JOHN RUSSELL, on a recent severe Thunder-storm in Belgium" Communicated by the Right Hon. Lord JOHN RUSSELL.

The writer states that the thunder-storm burst between seven and eight o'clock at night on Sunday the 19th of February, and was accompanied by an unusually heavy fall of snow throughout Belgium. Twelve churches were struck almost simultaneously, although at great distances from each other, namely, near Malines, Antwerp, Liege, Louvain, Charleroi, and Courtrai Those of Aerschot, Nazareth, Wesemael, fine old buildings, were totally destroyed, those of Puers, Lierre, Aerselaer, Lobbes, Walcourt, Marchienne au Pont, Liege, Courtrai, Moorslede, suffered more or less in the steeples or towers.

March 8, 1860

Sir BENJAMIN C. BRODIE, Bart, President, in the Chair

The following communications were read ·—

I. "On the Solar-diurnal Variation of the Magnetic Declination at Pekin." By Major-General Edward Sabine, R.A., Treas. and V.P.R.S. Received February 2, 1860

When the first year of hourly observations of the declination, January 1 to December 31st, 1841, was received at Woolwich from the Magnetic Observatory at Hobarton, and when means had been taken of the readings of the collimator scale at the several hours in each month, and these monthly means had been collected into annual means, it was found that the mean daily motion of the declination magnet at Hobarton presented, as one of its most conspicuous and well-marked features, a double progression in the twenty-four hours, moving twice from west to east, and twice from east to west, the phases of this diurnal variation were, that the north end of the magnet moved progressively from west to east in the hours of the forenoon, and from east to west in the hours of the afternoon, and again from west to east during the early hours of the night, returning from east to west during the later hours of the night the two easterly extremes were attained at nearly homonymous hours of the day and night, as were also the two westerly extremes, the amplitudes of the arcs traversed during the hours of the day were considerably greater than those traversed during the hours of the night

When, in like manner, the first year of hourly observations, July 1st, 1842, to June 30th, 1843, was received from the Toronto Observatory, and the mean diurnal march of the declination magnet was examined, it was found to exhibit phenomena in striking correspondence with those at Hobarton. At Toronto also a double progression presented itself, of which the easterly extremes were attained at nearly homonymous hours, as were also the westerly, whilst the hours of extreme elongation were nearly the same (solar) hours at the two stations, but with this distinction, that the hours at which the north end of the magnet reached its extreme *easterly* elongation at Hobarton were the same, or nearly the same, as those at which it reached its extreme *westerly* elongation at Toronto, and *vice versâ*. Pursuing, therefore, the ordinary mode of designating the direction of the declination by the north end of the magnet in the southern as well as in the northern hemisphere, the diurnal motion of the magnet may be said to be in opposite directions at Hobarton and Toronto, but if (in correspondence with our mode of speaking in regard to another magnetic element, the Inclination) the *south* end of the magnet is employed to designate the direction of the motion in the *southern* hemisphere, and the *north* end in the *northern* hemisphere, the apparent contrariety disappears, and the directions, as well as the times of the turning hours, are approximately the same at both stations.

The double progression in the diurnal variation, which was thus so distinctly and concurrently marked at stations so distant from each other, and at which the observations had been conducted with an elaborate care which would admit of no doubt as to the dependence to be placed on their general results, was at that time in great measure an unexpected and even a startling phenomenon. In the well-known description given by M. Arago (in the instructions drawn up for the voyage of the 'Bonite' in 1836) of the general phenomena of the diurnal variation in different parts of the globe, as then known, they are represented as consisting of a single progression only, with but one easterly and one westerly extreme, both occurring during the hours of the day, and no reference or allusion whatsoever is made to the existence of a double progression, or of a nocturnal interruption to the continuous motion in the one direction between the two extremes*. That the diurnal motion must be a consequence, in some way or other, of the sun's action, could not be doubted, from the fact that the period in which the variation takes place is a solar day, and whilst the progression was regarded as a single one in the twenty-four hours, it accorded sufficiently well with the prevailing notion, that the magnetic variations were produced by variations of temperature, to meet the general view, notwithstanding the grave doubts and dissents which from time to time had been expressed by those who more closely examined the phenomena of particular localities. As the existence of a well-marked double progression at some stations on the globe could, however, no longer be disputed, the difficulty which now presented itself was to explain in what way this apparently double action of the sun was produced.

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* From the omission on the part of M. Arago of any notice of a nocturnal feature, it might perhaps be inferred that the diurnal variation at *Paris* is actually, as described by him, a single progression. It seems very improbable, however,

On a careful examination of the diurnal motion of the declination magnet on *different days* of the years referred to at the commencement of this paper, it soon became obvious that, both at Hobarton and Toronto, many days occurred in which the diurnal march was a single progression, the nocturnal retrogression wholly disappearing, and that there were many more days in which this was more or less approximately the case It further appeared, on subsequently comparing the observations of the *same* years at both stations, that the days most distinguished by a large and even sometimes an extravagant interruption of the otherwise continuous *single* progression, were generally the *same days at both stations*, and by extending the comparison to other though less complete series of observations in other parts of the globe, these days were identified as those *on which magnetic storms had prevailed*, viz. days which had been distinguished by the occurrence of perturbations, often of very considerable magnitude, affecting simultaneously the magnetic elements in all parts of the globe as far as observation extended, presenting a remarkable uniformity in the effects produced at *contiguous* stations, but (as shown by the simultaneous observations at Toronto and Hobarton) manifesting a great variety both in the character and the amount of disturbance in parts of the globe distant from each other. To separate the observations affected by these exceptional and casual influences from the ordinary and what might be deemed

that this should be the case, since the observations at Greenwich and Kew have shown that the progression is double at those stations.

the normal position of the declination magnet, and to study the laws of each taken *separately*, as well as in their combination, became therefore a preliminary work to the right understanding of either That some such mode of examination would be required, had indeed been anticipated in the instructions drawn up with so much sagacity by the Committee of Physics of the Royal Society, for the guidance of those who should engage in the direction of the Colonial Magnetic Observatories then in contemplation. In the preface to those instructions, it is expressly stated that "the progressive and periodical magnetic variations are so mixed up with the *transitory* changes, that it will be impossible to separate them so as to obtain a correct knowledge and analysis of the *progressive* and *periodical*, without taking express account of and eliminating the transitory or casual" The difficulties which impeded, and which still impede *an entire* compliance with this instruction, viz the *perfect* elimination of the transitory and casual changes, were found to be very great The direction which the magnet assumes when it is under the influence of a perturbation of this nature, is not distinguishable from the direction assumed under the ordinary magnetic influence, by any other criterion yet known than by the magnitude of its deflection from the mean or normal position in the same month and at the same hour, the magnitude of the abnormal deflection may be thus taken, to a certain extent, as a means of recognizing the existence of a perturbing force, and it is the only one we possess. If we employ this criterion of magnitude as manifesting a disturbed observation, and separate the observations so disturbed from the others, we must still be aware that there may exist, and that probably there do exist, amongst the body of observations from which the large disturbances have been separated, some which may be affected by the same disturbing cause or causes operating in a minor degree, and assuming the disturbances to have different laws from the general body, the unseparated minor disturbances may still impede the perfect deduction of the laws of the other class with which they are so intermixed.

But though the criterion of magnitude may not enable us to effect a *complete* separation of the two classes, it will suffice to accomplish an approximation to that end, it will separate a sufficient body of disturbed observations,—disturbed beyond the limit of any other known influential cause,—to permit the laws of the disturbing action to be investigated, and when these laws are known, we are furnished with the means of making at least an approximate estimation of the influence exercised by the uneliminated minor disturbances on the laws which we may proceed to deduce for the class of observations from which we have not been able to effect their perfect separation

Adopting this method of partially eliminating the influence of the magnetic storms in the observations at Hobarton and Toronto, and proceeding in the first instance with the caution suitable to a first experiment, an unnecessarily high value (as it subsequently proved) was taken as that which should distinguish a perturbed observation, and consequently but a small body of disturbed observations was separated. On a recalculation of the diurnal variation after the elimination of these, and a comparison of the results with the diurnal variation obtained previously from the whole of the observations, the character of the influence of the magnetic storms was very manifest By the elimination of the larger disturbances, the interruption to a continuous progression from the afternoon of the one day to the morning of the following, was considerably diminished both in continuance and amount A smaller separating value was then taken, and consequently a larger body of disturbed observations was eliminated, the effect produced was a still further reduction of the nocturnal feature These first essays were sufficient to show that the mean effects of the magnetic storms on the declination magnet, both at Hobarton and at Toronto, attained a maximum in the early hours of the night, and constituted at both stations a very considerable part, if not the whole, of the nocturnal portion of the double progression which has been described. By still further diminishing the separating value, but still keeping it well within the limits in which no complication of disturbing causes would be hazarded, so little was found to remain of the nocturnal interruption, that I ventured, in the 1st volume of the 'Toronto Observations,' published in 1845, to express the opinion that "*if the whole influence of the magnetic storms could be eliminated from the observations, the residual portion of the diurnal variation would be a single progression with but one maximum and one minimum in the twenty-four hours.*"

The peculiar character of the magnetic storms (or disturbances as they are sometimes called), and the periodical laws exhibited in their mean effects, have been the subject of frequent investigations since 1845. It is not necessary to notice on this occasion the results of these further than as they are connected with the explanation of the phenomena of the diurnal variation, which forms the subject of this paper. It has been shown, by abundant evidence, that though apparently casual in the times of their occurrence, the magnetic storms nevertheless produce mean effects, which, when the observations of more than a very few days are combined, are seen to be of a highly systematic character in all parts of the globe where their effects have been examined:—that the mean deflections which they occasion have always their particular hours of extreme elongation, with continuous intermediate progression:—that these hours are different in different parts of the globe, exhibiting apparently every possible variety:—that the *disturbance diurnal variation*, as for distinction's sake it may be called, constitutes everywhere a sensible portion of the diurnal variation shown by the mean of the hourly observations from which no elimination of disturbed observations has been made:—that the diurnal variation so obtained is in fact a resultant of two diurnal variations superposed, both referable to the sun as their primary cause, but manifesting by the difference in the character of the effects produced, a distinction in the mode of operation to which they are severally due. The disturbance variation is caused by deflections which are only of occasional occurrence; the more regular solar diurnal variation is distinguished, on the other hand, by the regularity of its daily occurrence; and its hours of extreme elongation, or (as they may be more familiarly termed) its turning hours are the same, or nearly the same hours of local solar time in all parts of the globe, whilst those of the disturbance variation show almost every possible variety. The relative magnitudes or proportions of the two components differ also very greatly at different stations; and thus, by the operation of causes which as yet are but very imperfectly known, at localities where the magnetic storms are excessive, the disproportion of the components becomes excessive also, and the phases of the regular variation are rendered altogether subordinate to those of the disturbance variation. Until therefore the extension of observations shall give rise to and establish some general theory whereby the influence of the disturbances in different parts of the globe may be predicated, their particular laws at every station must be sought by a special investigation, and no conclusion in regard to either of the components of the diurnal variation is entitled to be viewed as final which has not been preceded by such an investigation.

It has appeared desirable to enter more at length into this preliminary statement than may at first sight be thought to be required by those who have followed the different stages of the inquiries referred to, because the interpretation, which was given so far back as 1845, of the diurnal variation at Toronto and Hobarton, has scarcely received the consideration which might seem due to a laborious and apparently successful analysis of the phenomena, and there are some eminent physicists who have framed or adopted theories for the explanation of the diurnal variation, in which theories the existence of a double progression as a universal and necessary phase is essentially implied. Amongst these, the most prominent perhaps, and the one which has obtained the widest circulation, is the theory of the R. P. A. Secchi, Director of the Observatory of the Collegio Romano, published originally in Italian in 1854 in the 'Correspondenza Scientifica' in Rome, translated into English in the edition of 1857 of the late Dr. Nichol's 'Cyclopædia of the Physical Sciences,' and more recently adopted in the third volume of M. de la Rive's 'Traité d'Electricité' In M Secchi's memoir, the diurnal variation, with its double movement in the day and night, is ascribed to the direct action of the sun as a distant and powerful magnet, influencing the magnetic needle at different stations on the globe in a manner contingent upon the direction of the magnetic meridian at each place, and producing extreme deflections to the East and to the West twice in the twenty-four hours, the turning hours being about six hours apart, and stated to be appropriately represented by a formula of two terms, one involving the sine of the hour-angle, and the other the sine of twice that angle: the phenomena of the double progression at Toronto and Hobarton are thus viewed by him as "Types of all that happens beyond the limits of the torrid zone."

If I have represented M. Secchi's views correctly, and I think I have done so, the question between the conformity to nature of his views and mine would be tested by the facts (when they should be known) of the diurnal variation at a station in the middle latitudes where the principal influence of the magnetic storms should take place, not in the hours of the *night*, but in those of the *day* According to my interpretation of the phenomena at Toronto and Hobarton, such a station ought to exhibit a single progression, according to M Secchi's, a double progression with turning hours about six hours apart Such a station would therefore furnish what might be deemed a *crucial* experiment In the extension of our experimental knowledge which might be expected to follow from the adoption by Her Majesty's Government of the recommendations of the Royal Society and of the British Association, which have been communicated to Lord Palmerston with so much earnestness of purpose, and with so just an appreciation of their importance, by His Royal Highness the Prince Consort, as President of the British Association, it had been anticipated that it would not be long before the evidence derivable from such a station would be secured to us I have found it, however, sooner than I had expected, or had hoped for, in the three years and ten months of hourly observations of the Declination at Pekin, from January 1, 1852, to October 31, 1855, made under the superintendence of M Scatchkoff, attached to the Russian Embassy at Pekin, and published by our distinguished foreign member, M. Kupffer, in the volumes of the 'Annales de l'Observatoire Physique Central de Russie' The results of these observations, as far as they bear on the questions of the general phenomena of the diurnal variation, and on the mode in which these may be explained, form the subject of the present communication

The examination of these observations was first undertaken by me for the purpose of ascertaining, as far as possible by their means, the precise epoch of minimum in the so-called decennial period of the magnetic storms With this view a separation was made of the larger disturbances in the usual manner, and their laws at Pekin investigated. In this process it was soon perceived that the hours of principal disturbance were those of the day, both in the easterly and in the westerly disturbance deflections, and on subsequently receiving from the computers the annual mean of the diurnal variation corresponding to the whole period of observation (in which the omission of disturbed observations during the hours of the night had been comparatively very inconsiderable), I was not surprised to find that it exhibited no trace of a double progression. The results were as follows:—

Local Astron Time.	Variation.	Local Astron Time	Variation	Local Astron. Time	Variation
h m	′	h m	′	h m	′
0 6	1 55 W	8 6	0 19 W	16 6	0 53 E.
1 6	2 26 W	9 6	0 10 W	17 6	0 57 E
2 6	2 32 W	10 6	0 00	18 6	0 94 E
3 6	1 65 W	11 6	0 15 E	19 6	1 51 E
4 6	1 21 W	12 6	0 39 E	20 6	2 06 E
5 6	0 65 W	13 6	0 45 E	21 6	2 10 E
6 6	0 29 W	14 6	0 49 E	22 6	1 24 E
7 6	0 21 W	15 6	0 52 E	23 6	0 20 W

If we examine these figures, we perceive that the motion from west to east, commencing at the turning hour between 1 and 2 in the afternoon, though comparatively slow during the hours of the night, is continuous and uninterrupted until the extreme easterly elongation is reached between 8 or 9 in the following morning, and that no other turning hours intervene between those of the extreme easterly between 8 and 9 A.M. and the extreme westerly between 1 and 2 P.M.

The phases of the solar-diurnal variation, as they are shown by the Pekin Observations, may be stated as follows.—The north end of the magnet is at its extreme eastern elongation about half-past 8 in the morning; at this hour it begins to move to the west, and moves rapidly in this part of its daily course, completing its whole movement in that direction in five hours, and reaching its extreme western elongation at about half-past 1 P.M. From this hour it returns, somewhat less rapidly than in its forenoon excursion, until about 6 P.M., when the rate of progression is considerably lessened, but continues in the same direction through the hours of the night, until about 5 A.M., when it again accelerates until the eastern extreme is attained, as already stated, about 8½ A.M. There is thus a very unequal division of time in the direction of the motion, which takes five hours in the progress from east to west, and nineteen hours in returning from west to east through the same arc. We find a more equal division of time if we regard the greater or less *rapidity* of the motion. there are about twelve hours in which the motion is comparatively quick, and twelve hours in which it is comparatively slow, the quick hours being those of the day, the slow hours those of the night.

Thus far the notice we have taken of the Pekin results has been limited to the diurnal variation which we find when we take an average of the *whole* year, and which we may theoretically suppose would take place in every month of the year if the sun were always in the plane of the equator But similar investigations had already made known to us the existence of a *semiannual inequality*, having opposite phases according as the sun has north or south declination, with turning epochs about the times of the solstices, and the phases passing into each other about the times of the equinoxes I have already, on a former occasion (Proceedings of the Royal Society, May 18, 1854), submitted to the consideration of the Society the concurrent evidence from three stations, Toronto, Hobarton, and St. Helena, of the existence of this inequality, and of the almost uniform character of its phases at those stations, from which I ventured to infer the probability that an inequality having a similar character would be found to be a general phenomenon I am now able to add to the evidence which was then adduced, a representation of the semiannual inequality at three additional stations, viz. at the Cape of Good Hope, of which the particulars in detail will be found in the 2nd volume of the 'St. Helena Observations,'—at the Kew Observatory, taken from the hourly tabulations from the photographic curves obtained by the self-recording declinometer at that station,—and at Pekin, as shown in the following tabular view —

Semiannual Means of the Solar-diurnal Variation at Pekin

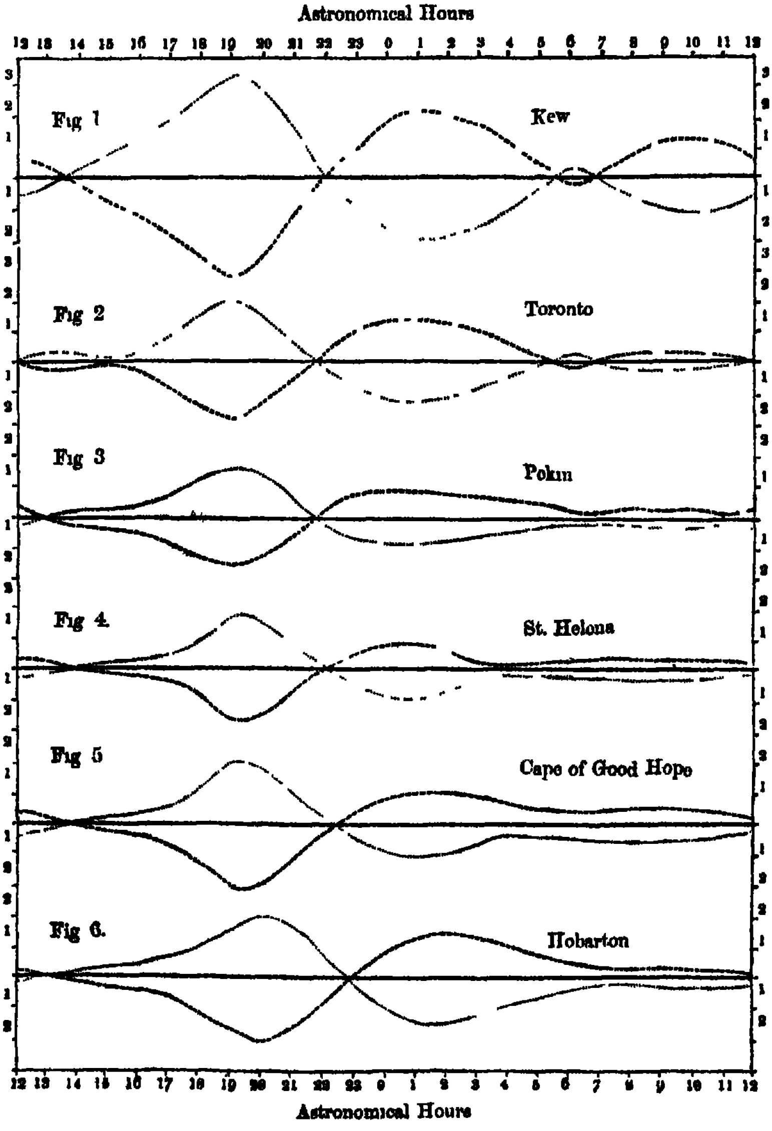
Local Astron Time.	April to Sept.	October to March	Local Astron Time	April to Sept.	October to March	Local Astron Time	April to Sept.	October to March
h m	′	′	h m	′	′	h m	′	′
0 6	2·34 W.	0·76 W.	8 6	0·40 W	0·02 E	16 6	0·84 E	0·21 E
1 6	3·09 W	1·44 W.	9 6	0·29 W	0·09 E	17 6	1·15 E	0·00
2 6	3·06 W	1·58 W	10 6	0·26 W	0·26 E	18 6	2·07 E	0·19 W
3 6	2·53 W	1·18 W.	11 6	0·08 W	0·39 E	19 6	3·04 E	0·02 W.
4 6	1·79 W	0·64 W	12 6	0·13 E	0·46 E	20 6	3·46 E	0·66 E
5 6	1·02 W.	0·27 W	13 6	0·43 E	0·47 E.	21 6	2·80 E.	1·40 E
6 6	0·43 W.	0·16 W.	14 6	0·63 E	0·36 E	22 6	1·15 E	1·33 E
7 6	0·34 W.	0·08 W	15 6	0·73 E	0·31 E.	23 6	0·76 W	0·36 E.

As the correspondence of such phenomena is often far better judged of by the eye, when exhibited in the form of curves, than by the comparison of tables, I have exhibited in a diagram the phases of the semiannual inequality at the six stations, by which it will be seen that they add their confirmation to the inference which I had previously drawn. With this additional evidence of its uniform character in different parts of the globe, it may be hoped that the claim of the semiannual inequality to be received as a successful generalization from a careful and comprehensive induction may be admitted, and that as an accession to our positive knowledge it may have a recognized place amongst the facts of the diurnal variation, which have to be accounted for in the theories which may be hereafter adduced for their physical explanation.

We now, therefore, recognize three classes of phenomena derived from three different sources, which are superposed in the diurnal variation obtained from the unreduced observations, and which for a proper understanding of the whole, require to be separated from each other by a proper analysis, so that the part due to each may be distinctly ascertained: these are—1st, the mean effects of the magnetic storms, 2nd, the semiannual inequality of the regular solar-diurnal variation, and 3rd, the mean solar-diurnal variation of the year into which the semiannual differences merge. The distinctive characteristics of the first, viz the disturbance diurnal variation, have already been stated in the early part of this paper, together with the evidence they supply of being due to some modification of the solar action,—justifying their being treated as distinct and separate from the affections which constitute the more regular variation. There are also distinctive characters in the phenomena of the semiannual inequality, and in those of the mean variation, which appear to point out a difference in the mode in which the primary cause operates in producing the two classes of phenomena. For the purpose of explaining this difference, we may employ, as more likely to be generally understood, the usual custom of referring all deflections, whether in the northern or the southern hemisphere, to the north end of the magnet; we say then that, in the mean variation, the directions of the deflection are uniform throughout the year in the middle latitudes of the one hemisphere, and (although opposite) are also uniform throughout the year in the middle latitudes of the other hemisphere, whilst in the semiannual inequality, the directions of the deflection are uniform in the two

Solar-diurnal Variation of the Magnetic Declination.—Semiannual Inequality

The Black line indicates the Mean Solar-diurnal Variation in the year
The Broken line indicates Semiannual Means, October to March
The fine dotted line indicates Semiannual Means, April to September



hemispheres, but opposite in the two half years In the one case the effects are hemispherical, in the other semiannual It is this peculiarity which gives to the "April to September" branch of the semiannual inequality its analogy with the diurnal variation which prevails throughout the year in the middle latitudes of the *northern* hemisphere, and to the "October to March" branch its analogy with the diurnal variation which prevails throughout the year in the middle latitudes *of the southern* hemisphere The analogies extend even to the small but apparently systematic difference which exists between the turning hours of the mean variation in the two hemispheres, and of the semiannual variation in the two half years The turning hours of the variation in the northern hemisphere, and of the "April to September" semiannual branch, appear to occur systematically about an hour earlier than those of the southern hemisphere, and of the "October to March" semiannual branch This is a connecting link which draws still nearer the analogies of which the broader features have been frequently noticed and commented upon, and is the more remarkable on account of the diversity which in other respects seems to distinguish the mode of operation by which the solar influence produces in the one case hemispherical difference with annual agreement, and in the other case semiannual difference with hemispherical agreement

Thus at Pekin, regarded as a station in the middle latitudes of the northern hemisphere, if we view the semiannual mean of the six months from April to September, we see repeated the general features of the annual mean, reinforced by the semiannual inequality of kindred character with itself, the deflections of both having the same direction at the same hours, the range becomes enlarged, but its characteristics are unchanged; the progression is still a single one, as is the case in the annual mean, with but one easterly and one westerly extreme, the hours of which are slightly earlier than those of the annual mean, by reason of a particular feature of the semiannual inequality spoken of above. When, on the other hand, we direct our attention to the semiannual mean from October to March, we see the consequence of the superposition upon the annual mean of the opposite semiannual inequality belonging to these months: this is most particularly shown in the effect produced upon the semiannual mean by the great semiannual loop which culminates about

6 or 7 A.M. This deflection, which is opposite in direction to that appropriate to the hemisphere, prevails over it so far as to interrupt the progression, which on the mean of the year is continuous, and to produce a secondary maximum at about 7 A.M. This opposite deflection to that which is normal in the hemisphere, taking place at the hours when the semiannual inequality is greatest, is a common feature whenever, in the middle latitudes of either hemisphere, the mean diurnal variation of the one hemisphere is combined with the semiannual inequality which has the opposite analogy. The hour of principal discordance between them is always nearly the same, being determined by that of the principal deflection on the semiannual curve, which, as seen in the diagram, is nearly identical in solar time in all parts of the globe. In the semiannual mean from October to March the principal turning hours are a little later than in the annual mean, just as we have seen above that, in the April to September mean, the turning hours are a little earlier than in the annual, and for the same reason. Finally, it is this combination of the hemispherical and semiannual effects which creates the differences we observe in the amount and hours of the solar-diurnal variation in the different months in the middle latitudes of both hemispheres.

There is one more feature of some importance to the general theory of the diurnal variation, which is illustrated by the Pekin observations, and requires a brief notice. A distinction has been elsewhere pointed out (Cosmos, English Translation, Longman's Edition, vol. iv. p. 504, Editor's Note) between the diurnal variation of the equatorial zone and that of the middle latitudes, consisting in the circumstance that in the equatorial zone the amount of the semiannual deflection is greater than that of the hemispherical deflection at the hours when they are opposed to each other, and by its preponderance *changes the character*, instead of *simply diminishing the amount*, of the hemispherical deflection. The change in the signs of the deflection at 6 and 7 A.M. in the semiannual mean from October to March at Pekin is an illustration of this peculiarity, and ought perhaps in strictness to cause Pekin to be included in the magnetically equatorial zone; but being only just within the border, it has been found more convenient to dwell on this occasion upon the features which it has in common with stations in the middle latitudes.

The diurnal variation at Pekin reaches its extreme deflections at the same hours of solar time, as is the case at the other stations in the northern hemisphere where the phenomena have been examined with equal care. This fact is not in accord with the opinions of those physicists who regard the solar action as conditioned in its exercise by the direction of the magnetic meridian at the particular station. In the different stations in the northern hemisphere, where the extreme deflections have been found to take place at the same hours of solar time, the differences in the direction of the magnetic meridian have not been less than 70°, equivalent to a difference of solar time of between four and five hours

I ought not to close this paper without adverting to the success which has attended Mr. Scatchkoff's employment of native Chinese as his assistants in the work of the Pekin Observatory, holding out as it does an encouraging example to Directors of Observatories who may be similarly circumstanced. A very close test of the care and fidelity with which observations have been made and recorded is furnished by the lunar-diurnal variation, deducible from them when they have been re-arranged under the lunar hours to which they severally belong Thus tested, the Pekin observations show no inferiority to those of other stations which have been similarly examined.

It is understood that the observations, which were discontinued at Pekin at the end of 1855, are about to be recommenced,or have been so already. It is greatly to be desired that hourly obervations of the Horizontal and Vertical Forces should be combined with those of the Declination at this important station. The self-recording apparatus of the three elements which has been in action at Kew during the last two years, has been found, by the reduction of its tabulated values at hourly intervals, to be in no respect practically inferior to the method of eye-observation, whilst it possesses many advantages which are peculiarly its own. The tabulation from the Photographic Curves, as well as the reductions, might be made, if more convenient, at the central Physical Observatory at St. Petersburgh.

March 15, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Robert Patterson, Esq., was admitted into the Society.

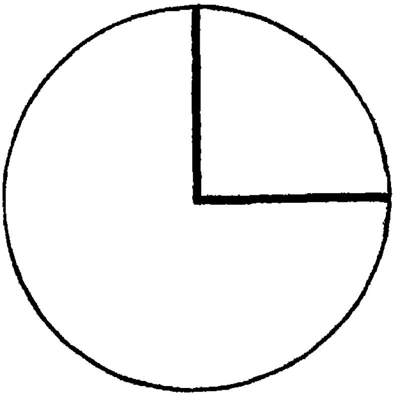
The following communications were read :—

I. "Analysis of my Sight, with a view to ascertain the focal power of my eyes for horizontal and for vertical rays, and to determine whether they possess a power of adjustment for different distances." By T. Wharton Jones, Esq., F.R.S., Professor of Ophthalmic Surgery in University College, London, &c. Received March 8, 1860.

Besides the well-known differences of sight in respect to farness and nearness, there are differences in respect to the power of the eyes of different persons to bring the rays of light to one exact focus

From observations and experiments in which I have for some time been engaged, I have been led to suspect that *astigmatism* or incapacity of the eye to collect all the rays of light which enter it to one exact focus, is, if not the rule of sight, at least of very common occurrence. I do not here refer to the cases in which astigmatism is of so exaggerated a character as to be a positive defect of sight.

It would be of great importance, both in a scientific and practical point of view, to possess some accurate data as to the frequency of the occurrence of astigmatism, but such can be obtained only by a number of different persons—qualified observers—contributing each an analysis of his own sight I have thought, therefore, that by bringing under the notice of the Royal Society an analysis of my own sight, some of the Fellows and others accustomed to exact observations might, perhaps, be induced to make similar contributions. The adjustment of the eyes for different distances being intimately connected with the question of *stigmatism* or *astigmatism*, I have included it in my analysis.



If I view a vertical and horizontal line, both equally strong and black, I see them with medium distinctness at the distance of about 10 inches.

At the distance of about 8½ inches, I see the vertical line with greater distinctness and better definition—the greatest distinctness and best definition my eyes are ca-

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* Continued from page 374.

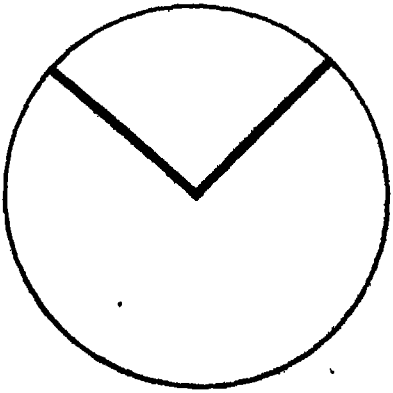
pable of; but the horizontal line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. At the distance of 12 inches, I see the horizontal line with the greatest distinctness and best definition my eyes are capable of, but the vertical line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. It thus appears that my eyes collect to a focus on the retina the rays which diverge horizontally at the distance of 8½ inches, and the rays which diverge vertically at the distance of 12 inches. Whilst seeing the vertical line with perfect distinctness and definition at the distance of 8½ inches, I cannot alter the adjustment of the eye so as to see the horizontal line more distinctly and the vertical one less distinctly, and *vice versâ*, whilst seeing the horizontal line perfectly defined at the distance of 12 inches, I cannot alter the adjustment of the eye so as to see the vertical line more distinctly and the horizontal one less

In short, I find that I have no power of altering the adjustment of my eyes I see vertical lines with perfect distinctness and definition only at the distance of 8½ inches, and horizontal lines with perfect distinctness and definition only at the distance of 12 inches, and both vertical and horizontal lines simultaneously with medium distinctness only at the distance of 10 inches.

At the distance of about 7 inches I see the vertical line with medium distinctness, but the horizontal line very indistinctly

At the distance of about 14 inches I see the horizontal line with medium distinctness, but the vertical line very indistinctly.

At a nearer distance than 7 inches I see both lines indistinctly, but the vertical less so than the horizontal At a further distance than 14 inches, on the other hand, I see both lines indistinctly, but the horizontal less so than the vertical.



If now I view two oblique lines, both of which are equally strong and black, I see both legs with medium distinctness at the distance of 10 inches.

At the distance of about 8½ inches I see the two oblique lines equally well, but not so distinctly as at the distance of 10 inches

At the distance of 12 inches I see the two oblique lines with much about the same distinctness as that with which I see them at the distance of 8½ inches

It thus appears that I cannot see either of the oblique lines with perfect distinctness and definition at any distance, but that I can see them both simultaneously distinctly enough at any distance from 8½ inches to 12. At a nearer distance than 8½ inches, or a further distance than 12 inches, the distinctness diminishes, and that equally for the two lines

I cannot by any adjustment of my eyes vary the distinctness with which I see the oblique lines at a given distance

The preceding analysis of my sight shows that my eyes are *not monostigmatic*, that is, are not capable of collecting all the rays of light which enter them to one exact focus. It shows, on the contrary, that my eyes are *distigmatic*, that is, they have each two distinct foci to which they bring the rays, viz one focus for horizontal rays, and one for vertical rays

The preceding analysis also shows that my eyes do not possess any intrinsic power of adjustment whereby they can bring to foci rays diverging from a nearer or further distance than the two distances above specified for horizontal and for vertical rays

It is true that I can see the different objects in a room distinctly enough without the aid of glasses, and that in the street or open country I can see objects distinctly enough for all practical purposes with the aid of concave glasses Nos. 2 and 3, but, critically speaking, the definition is far from being exact

Directing my eye to an object 2 or 3 feet from me, I see it distinctly enough whilst an object in the same field of view at the distance of 10 or 12 feet is at the same moment seen very indistinctly. If now, I direct my eye to the object at the distance of 10 or 12 feet, I see it distinctly enough, but the object at the distance of 2 or 3 feet now appears very indistinct.

This is commonly considered an evidence of adjustment of the eye to the two different distances There is, however, no real intrinsic adjustment in the case I see distinctly enough, either the nearer or the more distant object, merely because by directing my eye to it, its image falls on the central and most sensitive part of the retina, whilst the image of the other object falls on the circumferential and least sensitive part of the retina

It is to be observed that at neither the nearer nor the further distance, do I see the object exactly defined on directing my eye to it On directing my eye to the further object, I see it, of course, less defined than I do the nearer object when I direct my eye to it, but the difference is not at a glance very striking.

This experiment must not be confounded with another adduced by the late Professor Muller as a proof of the existence of an adjusting power in the eye. The experiment I refer to is as follows —

If we regard with one eye only (the other being closed) the ends of two pins placed one before the other at different distances in the line of the axis of the eye, one will be seen distinctly when the other appears indistinct, and *vice versâ* Both images lie in the axis of the eye, one over the other, and yet it depends on a voluntary effort, the exertion of which can be felt in the eye, whether the first or the second pin shall be seen distinctly "The two images of the pins," says Muller, "fall upon the same point of the retina, one lies over the other, and yet I see the nearer through the cloud-like image formed by the rays from the other more distant pin, and *vice versâ*"

If any person is able to see the phenomena here described, he is undoubtedly endowed with an adjusting power in his eye

I have never succeeded in seeing the phenomena myself

In viewing objects at different distances, the sight is no doubt aided by the movements of the eyebrows, eyelids, eyeballs, and pupils, but in this we have no example of adjustment properly so called, viz intrinsic adjustment

That the focal power of my eye may become slowly altered, for instance by prolonged examination of near and minute objects, and again slowly return to its former state, I am satisfied, but this, again, is no example of adjustment properly so called.

P S It would oblige me very much, if any one, into whose hands this paper may happen to fall, and who may take the trouble to analyse his sight in the manner herein described, would communicate to me the results of his observation on the following points —

1. The distance at which the vertical line is seen with the greatest distinctness and best definition

2 The distance at which the horizontal line is seen with the greatest distinctness and best definition —Or,

3 If there be no difference in the distance at which the vertical and horizontal lines are seen with the greatest distinctness and best definition —And, lastly,

4 Whether or not the observer can satisfy himself that he has the power of adjusting the eye, so as to be able to see the lines with perfect distinctness and definition at any other than one distance

N B If spectacles are worn, mention the kind of glasses—whether convex or concave—and their power

Note also if there be any difference in the sight of the two eyes

II "On the Light radiated by heated Bodies" By BALFOUR STEWART, Esq, A.M Communicated by J P GASSIOT, Esq. Received February 7, 1860

In two papers read before the Royal Society of Edinburgh in the years 1858 and 1859, and published in their Transactions, I have described some experiments on radiant heat, which would seem to involve an extension of Prevost's theory of radiation, known as the theory of exchanges

As the paper which I have now the honour to submit to this Society will detail analogous experiments on radiant light, I may be permitted briefly to refer to those points in my previous papers which are thus intimately connected with the present subject

In attempting to unfold the logical consequences of Prevost's theory, certain properties of radiant heat present themselves to our view, many of which are capable of experimental verification.

The following are some of these; and, for convenience-sake, I shall follow up the statement of each (before proceeding to the next) with a description of the analogous property of radiant light, as in this way the similarity which exists between heat and light will be most readily perceived.

In the first place, the heat radiated by a thin plate of any substance at a given temperature, is proportional to the absorptive capacity of that substance for the heat of that temperature, or, in few words, its radiation is equal to its absorption

Rock-salt, for instance, has a small absorptive capacity for heat of 212° F, and, in consequence, its radiation when heated to 212° F is comparatively small In point of fact, a plate of this substance, 0 18 inch thick, only gives out 15 per cent of the heat which lamp-black radiates at the same temperature Glass, on the other hand, absorbing nearly all the heat of 212° F which falls upon it, has at this temperature a radiation comparatively great, and nearly equal to that of lamp-black A similar law holds with regard to radiant light

If a piece of perfectly transparent glass be heated in an ordinary fire, removed to the dark, and there viewed, it will be found to emit scarcely any light, if the glass be slightly coloured, its radiation will be more copious, the amount of light given out, as far as I have been able to make the comparison, invariably depending upon the depth of colour or absorptive power of the glass for light, provided its colour stands heating A good way of performing this experiment is to heat a dark glass by the side of a colourless one, by means of a chemical tongs, in some uniform field of heat. When viewed in the dark together, the contrast is very striking between the bright light of the one and the bare visibility of the other

A stratum of heated gas may likewise be instanced as a substance which neither absorbs nor emits light to a sensible extent, and it has similar properties with respect to heat

Let us now proceed to another consequence of Prevost's theory. It is well known, from Melloni's experiments, that thin plates of various substances have the property of sifting the heat which falls upon them, they stop certain rays, and allow others to pass, the heat stopped being of one description, and the heat passed of another. Now, it may be shown to flow from the theory of exchanges, that the heat radiated by a thin plate of any substance at a given temperature is precisely that description of heat which the plate absorbs when heat of that temperature is allowed to fall upon it The heat which it absorbs being that kind of heat which has a difficulty in passing through it, if the heat which it radiates be of this description also, it follows that the heat given out by a plate of any substance will experience difficulty in passing through a screen of the same substance This we find to be the case thus, a plate of rock-salt 0 77 inch thick passes only 30 per cent of the heat from a thin plate of rock-salt heated to 212° F, whereas it will pass 75 per cent of heat from lamp black at that temperature The same thing holds with regard to glass A thin plate of crown glass will only pass half as much heat from heated crown glass as from heated lamp-black But this peculiar quality of the radiating plate is destroyed if we coat the side of it furthest from the screen with lamp-black, it then behaves precisely as lamp-black alone would do The reason of this is that the rays of heat given out by the glass are the equivalent of those which it absorbs from the lamp black behind, so that both together give out the same heat in quantity and quality as lamp-black would alone

We have here also analogous properties of light Let us take a number of differently coloured glasses With respect to the light of an ordinary fire, these may be divided into two groups, viz those which redden, and those which whiten the fire when we look through them The first group comprises red and orange glasses, the second group green and blue glasses

Glasses of the former group absorb the whiter, glasses of the latter group, the redder descriptions of light We should therefore expect red and orange glasses to give out, when heated, a peculiarly white light, and green and blue glasses a peculiarly red light A number of red and orange glasses have been found which fulfil this expectation. Among the reds, those coloured by gold, when removed from the fire and held in the dark, give out a milky-white, or even greenish light, and the orange glasses used by photographers do the same Other glasses, of a dingy red tint, give out, when heated, light slightly whiter than the ordinary light of their temperature, while there are others in which I have not, by this somewhat rude method of experimenting, been able to detect a sensible peculiarity of tint: yet this is not to be wondered at, if it be remembered that the following is the method in which the experiment is made. The glass under examination is held in a tongs, along with another glass, opaque or nearly so, which gives out light of the ordinary description They are heated together in the same field of heat, and then viewed in the dark as they cool During this process the tint of each changes, becoming somewhat redder as the temperature falls. A difference in the temperature of the two glasses might therefore cloak or even reverse the peculiar difference of tint which it is sought to establish, unless this is very marked This difficulty would be got over if we could by any means compare glasses of different tints at precisely the same temperature in the dark together, but I have not yet succeeded in contriving an apparatus for this purpose.

With respect to glasses that whiten the fire when used as screens, these all, without exception, as far as I have tried, give out a red description of light, and this is peculiarly remarkable in some light-green glasses

The following circumstance renders some glasses unfit for experiment They absorb nearly all the red light of low temperature, and it is only when the light rises to a white that a notable proportion is allowed to pass Now if the law be true that the radiation is equal to the absorption, these glasses should, at a low red heat, give out nearly the whole light belonging to their temperature, the same as if they were opaque It is only therefore when we raise the temperature that we can expect any result in the way of peculiarity of tint, for it is only then that the glasses, as screens, allow a notable proportion of light to pass, and that of a peculiar character, but the glass has now assumed a pasty condition, which renders it unfit to work with. Those glasses, therefore, are to be preferred that allow a considerable proportion of all the kinds of light to pass · for, just as in heat the radiation is most peculiar in rock-salt, which absorbs but little heat, so in light the best results are obtained by glasses that absorb only a small proportion of the light that falls upon them As a proof of this, I may mention that difference of tint is very noticeable in the process of spinning threads of coloured glass. The heated red glass thread has, when being spun, a pale green hue, while the green-glass thread has, under the same circumstances, a decidedly reddish appearance. These threads do not absorb much light, but what they do absorb is of a peculiar kind.

It has been mentioned that a screen of glass is peculiarly opaque for heat from glass; but if the side of the radiating plate furthest from the screen be coated with lamp-black, its heat now passes the glass screen as readily as ordinary heat of that temperature A similar fact is noticeable with regard to light A red glass, which, when heated and viewed in the dark, gives out a greenish light, while in the fire scarcely appears to differ in tint from the surrounding coals, and the same fact holds for all coloured glasses Ultimately they all appear to lose their colour in the fire as they approach in temperature the coals around them This may be explained thus —the red glass, for instance, still gives out its greenish light, but it passes red light from the coals behind it, in such a manner that the light which it radiates precisely makes up for that which it absorbs, so that we have virtually a *coal* radiation coming partly *from* and partly *through* the glass

Let us now consider Prevost's theory with regard to bodies of indefinite thickness One of its consequences was experimentally discovered by Leslie, viz that metals which are good reflectors of heat are very bad radiators As a variety of this experiment, I have endeavoured to show that a powdered diathermanous body will radiate less than bodies which are opaque for heat, powdered Thus, if a plate of table-salt have one side blackened, the white side will radiate only 83 per cent of that which the blackened side radiates at the temperature of 212° F No such difference is observed in sugar, which, though white for light, is black for heat of 212°

We have here also similar facts with regard to light If a pot of red-hot lead or tin be carried to the dark, and the dross scummed aside by means of a red-hot iron ladle, the liquid metal momentarily disclosed will appear less luminous than the surrounding dross, the difference being more observable in the case of tin, which has a higher reflecting power than lead Also, if a piece of platinum, partly polished and partly tarnished, be held above a flame in a dark room, the tarnished portion will shine much more brilliantly than the polished. Again, if we take a china cup with a white and black pattern, and heat it to a white heat in the fire, while there we shall not perceive much difference between the white and black of the pattern, but, if we bring it into a dark room, we shall perceive the black to shine much more brilliantly than the white This reversal of the pattern presents a very curious appearance.

Finally, it is a consequence of Prevost's theory and an experimental fact, that opaque bodies, generally speaking, radiate the same description of heat at the same temperature In like manner, the light which they radiate is of the same description at the same temperature, one body is not red while a second is yellow and a third white, but they are all either red or yellow, or white together

An analogy has thus been established between radiant heat and light in certain of their properties Now two opinions have been entertained with regard to light —

1st. Some have regarded it as differing from radiant heat only in wave length

2nd Others have regarded the two as physically distinct, although possessing many properties in common It has even been thought that some kinds of light have no heating effect on the bodies on which they fall

I cannot but think that the facts just stated countenance the former opinion rather than the latter for Prevost's theory consists of the three following hypotheses —

1st That if an enclosure of any kind be kept at a uniform temperature, any body placed within the enclosure, and surrounded by it on all sides, will ultimately attain that temperature.

2nd That all bodies are constantly giving out radiant heat, at a rate depending upon their substance and temperature, but independent of the substance or temperature of the bodies that surround them.

3rd. And, consequently, that when a body is kept at a uniform temperature, it receives back just as much heat as it gives out.

From these three assumptions may be deduced all the facts that have been stated with regard to radiant heat, but in the argument it is essential that the rays under consideration shall have the property of heating the bodies on which they fall, and by which they are absorbed If this be not granted the argument fails. Now radiant light, or those rays only that affect the retina, have been found to possess properties analogous to those which radiant heat thus possesses in virtue of its departure lowering the temperature of the body which it leaves, and its absorption raising that of the body on which it falls. If, therefore, we suppose all kinds of radiant light to have the property of raising (however little) the temperature of the body by which they are absorbed, the facts that have been stated in this paper regarding light may be shown to be a natural consequence of Prevost's theory of exchanges, but if, on the other hand, we do not admit that all the kinds of radiant light given out by heated bodies possess this property, then in that case those facts cannot be explained by Prevost's theory, but they will require a new theory to account for them

This circumstance induces me to think that all the descriptions of light radiated by heated bodies have the power of heating, more or less, those bodies by which they are absorbed. Viewing the matter in this light, I have constructed the following Table, in which the logical consequences of Prevost's theory are stated in the first column, while opposite these in the second column are detailed the different experiments which they serve to explain

Table of the consequences of Prevost's theory, and the facts which they explain

Consequences of Prevost's theory	Facts which these consequences explain.
The radiation of a thin plate or particle is equal to its absorption, and that for every description of heat—that is to say, in quality as well as in quantity	Rock-salt which absorbs little heat of 212° F, gives out little, while glass, which absorbs much, gives out much The heat radiated by rock-salt has great difficulty in passing through a screen of rock-salt—the heat radiated by glass in passing through a screen of glass Colourless glass, when heated, gives out little light, opaque glass a great deal Red glass, which absorbs the greenish rays, gives out greenish rays; while green glass, which absorbs the red rays, gives out red rays. When a plate of glass is coated on its further side with lamp-black, its heat is the same as lamp-black heat All coloured glasses appear to lose their colour in the fire
Those opaque bodies which reflect most, radiate least. Opaque bodies generally give out the same kind of rays at the same temperature these words also express the known fact	Metals radiate little, both of heat and light. Table-salt, which is white for heat of 212°, radiates less than sugar, which is black When a black and white china cup is heated in the fire and held in the dark, the black of the pattern is more luminous than the white

In conclusion, I may be permitted to remark regarding these laws of light, that from their simple nature some of them may have been observed before, but I think they are now for the first time connected with a theory of radiation.

Supplement (added March 7, 1860) —Since writing the above, the law which asserts that the absorption of a thin plate or particle is proportional to its radiation for every description of light, has received a very beautiful confirmation

In the Philosophical Magazine for this month, pages 194–196, Professor Stokes has noticed some very interesting experiments of M. Foucault, and also of Professor Kirchhoff M Foucault finds as the result of his experiments, "that the voltaic arc formed between charcoal poles presents us with a medium which emits the ray D of the solar spectrum on its own account, and which at the same time absorbs it when it comes from another quarter" Professor Kirchhoff, again, finds as the result of his experiments on the spectra of coloured flames, "that coloured flames, in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines, when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting."

We thus see that the same media which in a heated state emit rays of a certain refrangibility in great abundance, have also the property of stopping these very rays when they fall upon them from another quarter, or, in other words, their absorption of such rays is proportional to their radiation of them.

Supplement (added March 8, 1860) —The following fact noticed by Professor Kirchhoff is also in accordance with the theory brought forward in this paper

"The spectrum of the Drummond light," he remarks, "contains, as a general rule, the two bright lines D, if the luminous spot of the cylinder of lime has not long been exposed to the white heat, if the cylinder remains unmoved these lines become weaker, and finally vanish altogether. If they have vanished, or only faintly appear, an alcohol flame into which salt has been put, and which is placed between the cylinder of lime and the slit, causes two dark lines of remarkable sharpness and fineness to show themselves in their stead, but the Drummond light requires, in order that the lines D should come out in it dark, a salt-flame of lower temperature The flame of alcohol containing water is fitted for this, but the flame of Bunsen's gas-lamp is not With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves "

Now, when we heat a piece of ruby glass in the fire, we have an analogous phenomenon As long as the ruby glass is of a lower temperature than the coals behind it, the light given out is of a red description, because the ruby glass stops the green · the green here is precisely analogous therefore to the line D, which is stopped by an alcohol flame into which salt has been put Should, however, the ruby glass be of a much higher temperature than the coals behind it, the greenish light which it radiates overpowers the red which it transmits, so that the light which reaches the eye is green more than red. This is precisely analogous to what is observed when a Bunsen's gas-lamp with a little salt is placed before the Drummond light, when the line D is no longer dark but bright.

In fact, the law, "the absorption of a particle is equal to its radiation, and that for every kind of light," only applies to the case where the temperature of the particle is equal to that of the source of the light which passes through the particle If the temperature of the source of light be greater, one quality of light will predominate, if, on the other hand, the temperature of the particle be greater, another quality of light will predominate.

III. "On the Luminous Discharge of Voltaic Batteries, when examined in Carbonic Acid Vacua." By J P. Gassiot, Esq., F R.S. Received February 6, 1860.

On the 24th of May, 1859 (Proceedings, May 26, 1859), I communicated to the Royal Society a short notice of my having obtained the stratified discharge from a voltaic battery of 3520 elements charged with rain-water, and also with one of 400 elements charged with nitric and sulphuric acids, each cell of both batteries being insulated :—I stated also that with the latter (as I had previously shown with the former) spark discharges passed between two terminal copper plates through the air, before the completion of the circuit

The well-known luminous arc in air, as is usually obtained from an extended series of the nitric-acid battery, was very brilliant, but from the small size of the porous cells (3 inches long, ½ inch broad) containing the nitric acid, it was only tried by a momentary action With the water-battery I have never been able to obtain a continuous discharge in air similar to the voltaic arc, whether from points or plates, the discharges from this battery are invariably in the form of minute clearly defined, and separated sparks

Although the water-battery consisted of nearly nine times as many metallic elements as the nitric-acid batterv, it exhibited by the gold-leaf electroscope very little increased signs of tension

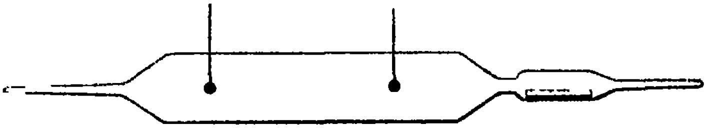
This is in accordance with the results obtained by me in 1844, when I showed that the tension of a single cell increased in force according to the chemical energy of the exciting liquid "In all the experiments I made, the higher the chemical affinities of the elements used, the greater was the development of evidence of tension" (Philosophical Transactions, 1844, part 1 p 52)

Recently, through the kind introduction of Professor Wheatstone, I have had an opportunity of experimenting with 512 series of Daniell's constant battery, and my present object is to present to the Royal Society a short account of the results I have obtained, and to describe the appearance and character of the voltaic discharge of these several batteries when taken in vacua.

The vacuum-tubes I used were prepared by means of carbonic acid (Philosophical Transactions, 1859, part 1. p 137) For the sake of reference I will denote these by the original numbers which I am accustomed to attach as the vacua are completed —in the following experiments these were 146, 187, 190, 196, 202, and 219.

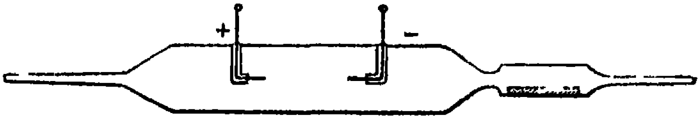
No. 146 is 24 inches long and 18 inches in circumference, and has a copper disc 4 inches diameter at one terminal and a brass wire at the other. this vessel is figured in my last communication. No 187 and 196 (see fig. 1) are each about 6 inches long and 1 inch diameter, with gas-coke balls ¼ inch diameter, attached to the hermetically sealed platinum wires, the wires being protected by glass tubing as far as the balls, placed inside the tube 3 inches apart.

Fig 1



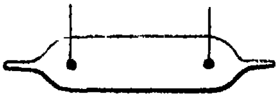
No 190 has brass wires, and No 202 silver wires attached to the platinum both these tubes are of the same dimensions as No 187 and 196 (fig 2) No 219 is 4 inches long, has gas-coke balls of

Fig 2



about $\frac{1}{8}$th of an inch in diameter, and 1 inch apart the caustic potash originally attached to this tube has been sealed off, the *form* is shown in fig 3

Fig 3

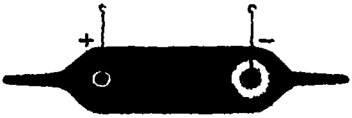


With the inductive coil the discharge in 146 exhibits a large cloud-like luminosity on the plate, which in these experiments was always made the negative terminal On the positive wire, minute luminous spots were visible At intervals, apparently by some sudden energetic action, flashes of bright stratified light would dart through the vacuum, but by carefully adjusting the contact breaker, the discharge could be made to pass, producing a white glow on the negative plate, without to the eye affording any appearance of an intermittent discharge.

In 187 and 196 the stratifications were narrow from the positive terminal, the negative ball being surrounded with a narrow halo of light similar to that in fig. 1 A, but smaller In 190 there was a red tinge on the cloud-like discharge near the positive terminal, on the approach of a magnet, two or three additional clouds were brought out, the negative wire being covered with a luminous glow extending to the sides of the tube parallel to the end of the negative wire

202 exhibited a remarkably well-defined cloud-like discharge, the several clouds being clearly and distinctly separated. In 219, the appearance of the discharge in this tube was similar to that in fig 3 A,

Fig 3 A



with a particularly brilliant glow around the negative ball, but without any stratifications from the positive

Having thus described the appearances of the induced discharge in these tubes, I now proceed to the description of experiments made in the same vacua,—1st, with the water-battery of 3520 cells, 2ndly, with the 512 series of Daniell's constant battery, and lastly, with 400 series of Grove's nitric acid battery

With the water-battery, as I have stated in my previous communication, the stratified discharge, similar in character to that of the inductive coil, was obtained, not only in 146 and the other tubes, but also through 146 and any one of the others at the same time.

From the risk of fracture, I have not ventured again to heat the potash in 146, but I have invariably found that whenever the potash in any of the other tubes was heated, the discharge from the water-battery, instead of increasing in distinctness and brilliancy, entirely ceased.

The discharge from the water-battery through each of the tubes had the appearance of being continuous, and the needle of the galvanometer, placed on the circuit, showed a steady deflection. To test whether the discharge was continuous, I attached No. 219 to my rotating apparatus (Philosophical Transactions, 1859, part 1. p 158); the discharges were then clearly separated, so that even with this apparatus they were shown to be intermittent

As the water in the battery, after a lapse of some weeks, had partially evaporated, the action was so reduced that it would no longer pass through any of the vacuum-tubes except 219, in which it assumed the appearance as in fig. 3 B In this state it remained for three or four weeks, and whenever from change of temperature moisture was deposited on the surface of the glass tubes, the luminous discharge

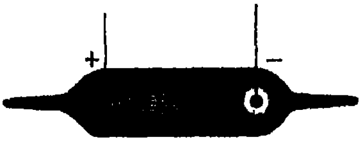


Fig 3 B

disappeared when the tubes were touched by the hand or by a wire, reappearing as the hand or wire was withdrawn A portion of the discharge from this battery was evidently lost by insufficient insulation, reduced by accumulated dust and moisture I have not the requisite facilities at command, but a thoroughly well-insulated battery of 3000 or 4000 series would produce effects well worthy of examination

Daniell's Constant Battery

On the 27th of December, 1859, by the introduction of Professor Wheatstone, I had the opportunity of experimenting with the large battery belonging to the Telegraph Company at its Factory, Camden Town, Messrs Wheatstone, Latimer Clarke, and Bartholomew were present. My object was to ascertain whether a luminous discharge could be obtained through the vacuum-tubes from the battery, which consists of 512 series of Daniell's elements, zinc and copper. The zinc plates are 2 by 3½ inches, the copper 3½ by 3½ inches, the cells are insulated in series of 10 and 12, suspended on trays by gutta-percha bands. I experimented with three of my vacuum-tubes, Nos. 187, 196, and 219, with 187 and 196, no sign of any luminous discharge could be observed, the electroscopes (Peltier's and a gold-leaf), by the tension, showed that the current did not pass

With 219 the brilliant glow around the negative ball, as is shown in the same tube by the induction coil, was visible with very trifling luminosity on the positive I have attempted to show this appearance in fig 3 A

Various lengths, viz. 110 yards, 1, 2, 4, 8 and 16 miles of covered copper wire, the 110 yards covered with india-rubber, and the other lengths with gutta-percha, are kept immersed in a water-tank for the purpose of experiments at the factory, an opportunity was therefore offered of testing the action of the battery on these wires by means of vacuum-tubes. This immersed wire is designated the cable, acting in this respect in a manner analogous to submarine wires.

The general results obtained, and repeatedly verified, may be briefly stated as follows:—From the 110 yards to the greatest length of 16 miles, it took from half to one and one quarter of a second for the cable to receive as much of the charge of the whole battery as could pass through the vacuum-tubes 219, the time being denoted by the appearance of the luminosity in the negative ball (see fig 3 A)

With 110 yards the discharge of a charge previously given to the cable was instantaneous, it appeared to be nearly momentary with one mile, and the time then progressively increased according to the length of the cable previously charged, until with the 16 miles it took one and a quarter to one and a half seconds before the luminous glow on the ball in the vacuum-tube disappeared

It was beautiful to see the regularity with which the glow appeared and disappeared in these experiments, first at one terminal and then at the other, according as the cable was charged or discharged

After the cable had received the discharge to the greatest intensity that could be obtained through the tube 219, the full charge of the battery was then completed by cutting off the tube from the circuit by means of a wire On removing the wire, and substituting the earth for the battery, a discharge took place, but a residuary charge was always found, which could not pass through the vacuum. If this residuary was allowed to remain in the cable, and the battery again substituted for the earth, no additional charge could be made to pass from the battery through the vacuum-tube, but so soon as this residuary was discharged, the cable again became charged through the tube as before*.

We were particularly fortunate with these experiments, for on Mr. Wheatstone testing the capability or power of the battery, he ascertained that on taking from it only 32 cells, thus reducing the number to 480, the discharge could not pass through the vacuum-tube.

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* If one of the wires of the vacuum-tube No 219 is connected with the inner coating of a Leyden jar, and the other with the prime conductor of an electrical machine, when the machine is excited a luminous glow will be observed round one of the balls, similar to that obtained during the charging of the cable by the voltaic battery, and the jar will become gradually charged. On the excitation of the electrical machine being stopped, if a pointed wire is presented to the prime conductor, the jar will be gradually discharged, and the luminous glow appears on the *other* ball of the vacuum tube, if several similar tubes are arranged in series, and the jar is discharged through longer carbonic-acid vacua, the striæ can be obtained in the latter

Grove's Nitric-Acid Battery

Each set of elements in the battery were inserted in a glass vessel with a stem 6 inches long, the stems were carefully cleaned and dried These precautions, with the high chemical affinity of the elements, raised the tension of each terminal, as denoted by gold leaf electroscopes, to nearly that of the larger series of the water-battery A succession of spark-discharges could be taken between the copper discs of my micrometer-electrometer, one disc being attached to the zinc, and the other to the platinum end of the battery

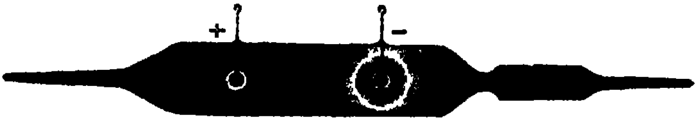
In the following experiments, the different vacuum-tubes were introduced between one of the copper discs and the battery, as also a galvanometer By this arrangement the circuit could be gradually completed without any risk of disarranging the apparatus, and the spark discharged obtained before the copper discs of the micrometer-electrometer came into contact

" In 146, on the completion of the current, the discharge of the battery passed with a display of magnificent strata of most dazzling brightness On separating the discs by means of a micrometer screw, the luminous discharges presented the same appearance as when taken from an induction coil, but brighter On the copper plate in the vessel there was a white layer, and then a dark space about one inch broad, then a bluish atmosphere, curved like the plate, evidently three negative envelopes on a great scale When the plate was positive, the effect was comparatively feeble." The preceding is an extract from notes made by the Rev Dr T. R Robinson, when he first witnessed the experiments in my laboratory, on the 5th of August, 1859 With the same vacuum I have always obtained similar results

In 187 and 196 (fig 1, with carbon-balls in the tubes) the discharge of the nitric-acid battery elicits intense heat, and probably changes the condition of the vacuum. On the 5th of August, 1859, " the discharge in 187 presented a stream of light of intolerable brightness [I again quote from Dr. Robinson's notes], in which, through the plate of green glass, with which he observed the phenomena, strata could be observed. This soon changed to a sphere of light on the positive ball, which became red-hot, the negative being surrounded by magnificent envelopes, whilst with the horseshoe magnet the positive light was drawn out into strata The needle of a galvanometer in circuit was violently deflected, and the polarity reversed, settling at a deflection of 45° On heating the potassa, the discharge again bursts into a sun-like flame, subsequently subsiding into three or four large strata, of a cloud-like shape, but intensely bright "

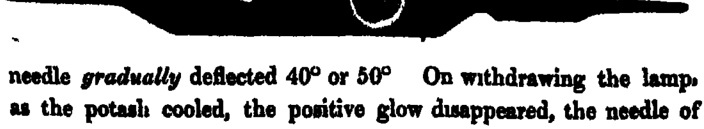
On a subsequent occasion I found that the discharge did not pass for about a minute after the circuit was completed, when a fine glow appeared on the negative ball, fig 1 A Around the positive ball there was a trifling glow, in a few seconds a momentary brilliant flash, and the discharge ceased The potash was then again heated, the large negative glow reappeared, followed almost instantly by a remarkably brilliant stratified discharge, with intense chemical action in the battery, denoted by the evolution of nitrous fumes, at this moment I separated the discs of the micrometer-electrometer, and thus broke the circuit

Fig 1 A



I now arranged the apparatus by attaching gold-leaf electroscopes to both terminals, and introduced the galvanometer, so as to enable me to examine more carefully the action that would take place when the potassa was heated On heating the potassa, the fine negative (fig. 1 A) was again developed, the leaves of the electroscope did not close, but as the negative glow increased, the needle of the galvanometer was *suddenly* deflected, immediately (although the glow continued) returning to zero, as more heat was applied a small globe of light appeared on the positive (fig 1 B), was visible, and the needle *gradually* deflected 40° or 50° On withdrawing the lamp, as the potash cooled, the positive glow disappeared, the needle of

Fig 1 B



the galvanometer receded, the glow on the negative remaining more or less brilliant, this action and reaction alternating as the heat of the lamp was applied or withdrawn from the potash

When the heating of the potash was further increased, four or five cloud-like and remarkably clear strata came out from the positive (fig 1 C), and these were quickly followed by a sudden discharge of the most dazzling brightness, which remained for several seconds. The stratifications, which were conical in shape, I have endeavoured, although very faintly, to depict in fig 1 D The needle of the galvanometer was suddenly and violently deflected, striking with considerable force the two corks placed to protect it on the compass card At the instant this discharge took place, and not before, the leaves of the electroscopes collapsed This, with the intense chemical action observable in the battery, proved that the entire current was passing

Fig 1 C

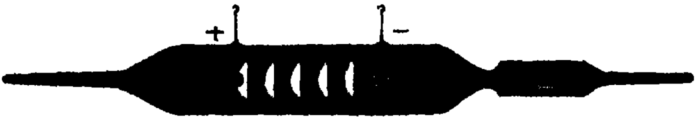
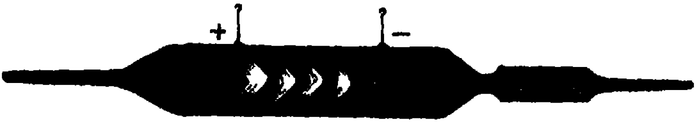


Fig 1 D



The preceding experiment was repeated with tube No. 196, with nearly the same results, the needle first deflecting 40° and then 80°. On further heating the tube, the same sudden intense stratified light appeared, after which the discharge ceased.

No 187 was then replaced in the circuit, and the same phenomena, as already described, were obtained

I now again avail myself of Dr Robinson's notes of the experiments made on the 5th of August Tube 190 is of the same dimensions as 187 and 196, but instead of the coke balls, it has brass wires attached to the platinum In this tube (190) the luminous discharge did not appear until the caustic potassa was heated, when most dazzling strata were observed. Dr Robinson says,—" I had to use a dark-green glass to examine the strata, as I was observing, the last strata rolled leisurely away, like a globe of light, from the others, to the negative glow, in which it appeared to dissolve As the potassa cooled, the strata shrunk up and dissolved at the positive wire, as did the glow, and when the dark negative reached the point, all luminosity ceased "

On a subsequent occasion the first discharge in the tube (190) exhibited a fine purple negative glow, with two tawny cloud-like stratifications at the positive This discharge exhibited the fluorescence of the glass tube in a remarkable manner. The circuit was then broken, on again completing it, I was somewhat surprised at finding the luminous discharge was no longer perceptible I then slightly heated the *tube* with a spirit lamp, the stratified discharge reappeared, the needle of the galvanometer deflected about 65°, but the leaves of the electroscopes were very slightly affected On separating the plates of the micrometer electrometer, sparks passed, the stratifications in 190 became confused, intermingling with each other, and no longer presenting that clearness of definition which I have described.

With tube 202, which is of the same dimensions, with silver in lieu of brass terminal wires, there was not any discharge from the battery until the potash was heated At first it presented a fine white glow on the negative wire, then one of a tawny red colour on the positive, and on heat being applied, stratifications, as in 190, were observed

The battery was then connected to the wires of tube 219, the same with which the experiments with Daniell's battery were made. This showed the luminous negative as in fig 3 A, but more brilliant than with the constant battery, producing bright scintillations at the positive, as if particles of the carbon were fused and thrown off.

By the preceding experiments, I ascertained that a disruptive or spark-discharge could be obtained in air from the nitric-acid, as well as from the water-battery, and that when these discharges were passed through the highly attenuated matter contained in carbonic acid vacua, the same luminous and stratified appearance was produced as by an inductive coil, a proof that whatever may be the cause of the phenomena, it could not arise from any peculiar action of that apparatus

With the ordinary arrangement of the voltaic battery, in which the insulation of the cells is disregarded, the luminous discharge is usually obtained by completing the circuit, and then separating the terminal wires or the charcoal points, the length of the arc being in relation to the number of the elements of the battery

With the water-battery I have never been able to obtain the slightest appearance of an arc-discharge, for whether the terminals of the battery were the plates of my micrometer-electrometer, metallic wires, or charcoal, the result was the same, viz clearly separated and distinct sparks, which continued until the water in the cells had nearly evaporated With four hundred (each cell being insulated) of Grove's nitric-acid battery, similar spark-discharges were obtained between the plates of my micrometer-electrometer, without producing the voltaic arc, although by a momentary completion of the circuit it was obtained with great brilliancy

In carbonic acid vacuum-tubes, and particularly in those in which carbon-balls were inserted, the disruptive, as well as the voltaic arc discharge (under the conditions described) was obtained from the nitric-acid battery, and in both instances the stratified appearance was observed, the difference being, that with the arc-discharge the stratifications were far more vivid and brilliant, and the discharge itself evidently more energetic With the disruptive discharge, the needle of the galvanometer was but slightly deflected, nor could any apparent chemical action be observed in the battery, but instantly on the production of the arc-discharge, the needle of the galvanometer was strongly deflected, and in all the cells of the battery it was evident that intense chemical action had been produced

If carbon-balls are attached to the wires of a carbonic acid vacuum-tube, the arc-discharge is obtained from a nitric-acid battery whenever the potash is heated. This process facilitates the discharge, and assists the disintegration of the carbon particles, and these in a minute state of division are subsequently found attached to the sides of the glass It is these particles which produce the arc discharge, with its intense vivid light so suddenly observed, with far more brilliant effects than the usual stratified discharge During its passage the conducting power of the vacuum-tube is found to be greatly enhanced, as shown by the galvanometer, the electroscope, and the intense chemical action in the battery.

The same explanation that I ventured to offer in the Bakerian Lecture for 1858, as to the cause of the stratified discharge arising from the impulses of a force acting on highly attenuated but resisting media, is also applicable to the discharge of the voltaic battery in vacua, while the fact of this discharge, even its full intensity having been now ascertained to be also stratified, leads me to the conclusion, that the ordinary discharge of the voltaic battery, under every condition, is not continuous, but intermittent, that it consists of a series of pulsations or vibrations of greater or lesser velocity, according to the resistance in the chemical or metallic elements of the battery, or the conducting media through which the discharge passes

March 22, 1860

Sir BENJAMIN C. BRODIE, Bart, President, in the Chair

In accordance with the notice given at the last meeting, the Right Honourable Edward, Lord Belper, was proposed for immediate ballot, and the ballot having been taken, his Lordship was declared duly elected.

The following communications were read —

I "On the Theory of Compound Colours, and the Relations of the Colours of the Spectrum." By J. Clerk Maxwell, Esq., Professor of Natural Philosophy, Marischal College and University, Aberdeen Communicated by Professor Stokes, Sec. R.S. Received December 27, 1859.

(Abstract)

Newton (in his 'Optics,' Book I part ii prop 6) has indicated a method of exhibiting the relations of colour, and of calculating the effects of any mixture of colours He conceives the colours of the spectrum arranged in the circumference of a circle, and the circle so painted that every radius exhibits a gradation of colour, from some pure colour of the spectrum at the circumference, to neutral tint at the centre The resultant of any mixture of colours is then found

by placing at the points corresponding to these colours, weights proportional to their intensities, then the resultant colour will be found at the centre of gravity, and its intensity will be the sum of the intensities of the components.

From the mathematical development of the theory of Newton's diagram, it appears that if the positions of any three colours be assumed on the diagram, and certain intensities of these adopted as units, then the position of every other colour may be laid down from its observed relation to these three Hence Newton's assumption that the colours of the spectrum are disposed in a certain manner in the circumference of a circle, unless confirmed by experiment, must be regarded as merely a rough conjecture, intended as an illustration of his method, but not asserted as mathematically exact From the results *of the present investigation, it appears that the colours of the* spectrum, as laid down according to Newton's method from actual observation, lie, not in the circumference of a circle, but in the periphery of a triangle, showing that all the colours of the spectrum may be *chromatically* represented by three, which form the angles of this triangle

	Wave-length in millionths of Paris inch
Scarlet	2328, about one-third from line C to D,
Green	1914, about one-quarter from E to F,
Blue	1717, about half-way from F to G

The theory of three primary colours has been often proposed as an interpretation of the phenomena of compound colours, but the relation of these colours to the colours of the spectrum does not seem to have been distinctly understood till Dr Young (Lectures on Natural Philosophy, Kelland's edition, p 345) enunciated his theory of three primary sensations of colour which are excited in different proportions when different kinds of light enter the organ of vision. According to this theory, the threefold character of colour, as perceived by us, is due, not to a threefold composition of light, but to the constitution of the visual apparatus which renders it capable of being affected in three different ways, the relative amount of each sensation being determined by the nature of the incident light If we could exhibit three colours corresponding to the three primary sensations, each colour exciting one and one only of these sensations, then since all other colours whatever must excite more than one primary sensation, they must find their places in Newton's diagram within the triangle of which the three primary colours are the angles

Hence if Young's theory is true, the complete diagram of all colour, as perceived by the human eye, will have the form of a triangle

The colours corresponding to the pure rays of the spectrum must all lie within this triangle, and all colours in nature, being mixtures of these, must lie within the line formed by the spectrum If therefore any colours of the spectrum correspond to the three pure primary sensations, they will be found at the angles of the triangle, and all the other colours will lie within the triangle

The other colours of the spectrum, though excited by uncompounded light, are compound colours, because the light, though simple, has the power of exciting two or more colour-sensations in different proportions, as, for instance, a blue-green ray, though not compounded of blue rays and green rays, produces a sensation compounded of those of blue and green

The three colours found by experiment to form the three angles of the triangle formed by the spectrum on Newton's diagram, *may* correspond to the three primary sensations

A different geometrical representation of the relations of colour may be thus described Take any point not in the plane of Newton's diagram, draw a line from this point as origin through the point representing a given colour on the plane, and produce them so that the length of the line may be to the part cut off by the plane as the intensity of the given colour is to that of the corresponding point on Newton's diagram In this way any colour may be represented by a line drawn from the origin whose direction indicates the quality of the colour, and whose length depends upon its intensity The resultant of two colours is represented by the diagonal of the parallelogram formed on the lines representing the colours (see Prof Grassmann in Phil Mag April 1854)

Taking three lines drawn from the origin through the points of the diagram corresponding to the three primaries as the axes of coordinates, we may express any colour as the resultant of definite quantities of each of the three primaries, and the three elements of colour will then be represented by the three dimensions of space

The experiments, the results of which are now before the Society, were undertaken in order to ascertain the exact relations of the colours of the spectrum as seen by a normal eye, and to lay down these relations on Newton's diagram The method consisted in selecting three colours from the spectrum, and mixing these in such proportions as to be identical in colour and brightness with a constant white light. Having assumed three standard colours, and found the quantity of each required to produce the given white, we then find the quantities of two of these combined with a fourth colour which will produce the same white We thus obtain a relation between the three standards and the fourth colour, which enables us to lay down its position in Newton's diagram with reference to the three standards

Any three sufficiently different colours may be chosen as standards, and any three points may be assumed as their positions on the diagram. The resulting diagram of relations of colour will differ according to the way in which we begin, but as every colour-diagram is a perspective projection of any other, it is easy to compare diagrams obtained by two different methods

The instrument employed in these experiments consisted of a dark chamber about 5 feet long, 9 inches broad, and 4 deep, joined to another 2 feet long at an angle of about 100° If light is admitted at a narrow slit at the end of the shorter chamber, it falls on a lens and is refracted through two prisms in succession, so as to form a pure spectrum at the end of the long chamber Here there is placed an apparatus consisting of three moveable slits, which can be altered in breadth and position, the position being read off on a graduated scale, and the breadth ascertained by inserting a fine graduated wedge into the slit till it touches both sides

When white light is admitted at the shorter end, light of three different kinds is refracted to these three slits When white light is admitted at the three slits, light of these three kinds in combination is seen by an eye placed at the slit in the shorter arm of the instrument. By altering the three slits, the colour of this compound light may be changed at pleasure.

The white light employed was that of a sheet of white paper, placed on a board, and illuminated by the sun's light in the open air, the instrument being in a room, and the light moderated where the observer sits.

Another portion of the same white light goes down a separate compartment of the instrument, and is reflected at a surface of blackened glass, so as to be seen by the observer in *immediate contact* with the compound light which enters the slits and is refracted by the prisms

Each experiment consists in altering the breadth of the slits till the two lights seen by the observer agree both in colour and brightness, the eye being allowed time to rest before making any final decision In this way the relative places of sixteen kinds of light were found by two observers Both agree in finding the positions of the colours to lie very close to two sides of a triangle, the extreme colours of the spectrum forming doubtful fragments of the third side They differ, however, in the intensity with which certain colours affect them, especially the greenish blue near the line F, which to one observer is remarkably feeble, both when seen singly, and when part of a mixture, while to the other, though less intense than the colours in the neighbourhood, it is still sufficiently powerful to act its part in combinations One result of this is, that a combination of this colour with red may be made, which appears red to the first observer and green to the second, though both have normal eyes as far as ordinary colours are concerned; and this blindness of the first has reference only to rays of a definite refrangibility, other rays near them, though similar in colour, not being deficient in intensity. For an account of this peculiarity of the author's eye, see the Report of the British Association for 1856, p 12.

By the operator attending to the proper illumination of the paper by the sun, and the observer taking care of his eyes, and completing an observation only when they are fresh, very good results can be obtained. The compound colour is then seen in contact with the white reflected light, and is not distinguishable from it, either in hue or brilliancy, and the average difference of the observed breadth of a slit from the mean of the observations does not exceed $\frac{1}{80}$ of the breadth of the slit if the observer is careful. It is found, however, that the errors in the value of the sum of the three slits are greater than they would have been by theory, if the errors of each were independent; and if the sums and differences of the breadth of two slits be taken, the errors of the sums are always found greater than those of the differences This indicates that the human eye has a more accurate perception of differences of hue than of differences of illumination

Having ascertained the chromatic relations between sixteen colours selected from the spectrum, the next step is to ascertain the positions of these colours with reference to Fraunhofer's lines. This is done by admitting light into the shorter arm of the instrument through the slit which forms the eyehole in the former experiments A pure spectrum is then seen at the other end, and the position of the fixed lines read off on the graduated scale. In order to determine the wave-lengths of each kind of light, the incident light was first reflected from a stratum of air too thick to exhibit the colours of Newton's rings The spectrum then exhibited a series of dark bands, at intervals increasing from the red to the violet The wave-lengths corresponding to these form a series of submultiples of the retardation, and by counting the bands between two of the fixed lines, whose wave-lengths have been determined by Fraunhofer, the wave-lengths corresponding to all the bands may be calculated, and as there are a great number of bands, the wave-lengths become known at a great many different points

In this way the wave-lengths of the colours compared may be ascertained, and the results obtained by one observer rendered comparable with those obtained by another, with different apparatus. A portable apparatus, similar to one exhibited to the British Association in 1856, is now being constructed in order to obtain observations made by eyes of different qualities, especially those whose vision is dichromic

II "On the Insulating Properties of Gutta Percha." By FLEEMING JENKIN, Esq. Communicated by Professor WILLIAM THOMSON Received February 9, 1860

(Abstract)

The experiments described in this paper were undertaken with the view of determining the resistance opposed by the gutta-percha coating of submarine cables at various temperatures to the passage of an electric current

The experiments were made at the works of R S Newall and Co, Birkenhead The relative resistance of the gutta percha at various temperatures was determined by measuring the loss on short lengths

immersed in water. These experiments are described in the first part of the paper. The absolute resistance of gutta percha has been calculated from the loss on long submarine cables. These experiments and calculations are described in the second part of the paper.

PART I

The loss of electricity was measured upon three different coils, each one knot in length. One was covered with pure gutta percha, the two remaining coils were covered with gutta percha and Chatterton's compound The coils were kept at various temperatures by being covered with water in a felted tub, and the water was maintained at a constant temperature for twelve or fourteen hours before each experiment

The loss or current flowing from the metal conductor to earth through the gutta-percha coating was measured on a very delicate sine-galvanometer The loss from the connexions when the cable was disconnected, was measured in a similar manner. The electromotive force of the battery employed was on each occasion measured in the manner described by Pouillet Corrections due to varying electromotive force and loss on connexions were made on the result of each experiment

A remarkable and regular decrease in the loss was observed for some minutes after the first application of the battery to the cable, a phenomenon, which the author thinks may be due to the polarization of the molecules of gutta percha, or of the moisture contained in the pores of the gutta percha. The loss was therefore measured from minute to minute for five minutes, with each pole of the battery.

Nineteen tables containing the results, with the reductions and curves representing the results, accompany the paper The following results were obtained from the first coil, this was prepared with Chatterton's patent compound. With a negative current between the limits of 50° and 80° Fahrenheit, the decrease of resistance is sensibly constant for equal increments of temperature, and the increase of resistance due to continued electrification is also nearly constant. At 60° the resistance increases about 20 per cent in five minutes from this cause With a positive current, similar results appear between the temperatures of 50° and 60°, but the resistance is somewhat greater than with the negative current The extra resistance due to continued electrification is unchanged by a change in the sign of the current Above the temperature of 63° great irregularities occur in the observations, which could not even be included in regular curves The difference in the resistance of the gutta percha coating when the copper is positively and negatively electrified, may be caused by the contact between the resinous compound and the copper no such difference was observed when pure gutta percha was in contact with the copper

The curves resulting from the experiments on the second coil, which was covered with pure gutta percha, present an entirely different character from those resulting from the first coil The copper and gutta percha were of the same size in these two coils The resistance of pure gutta percha at low temperatures is greater than that of the compound covering At 65° the resistance of the two coverings is equal, at higher temperatures the resistance of pure gutta percha diminishes extremely rapidly The curves obtained with positive and negative currents are identical up to about 75°, a slight difference occurs above this temperature, which may have been accidental The extra resistance is less with pure gutta percha than with the compound, it increases slightly at high temperatures, and is not affected by a change in the sign of the current

The curves derived from the experiments on the third coil, which contained a smaller proportion of Chatterton's compound than the first coil, appear in some respects intermediate between those derived from the first and second coils. The extra resistance due to continued electrification was still greater in this coil than in the others 40 per cent. of the entire resistance is at 70° due to this cause. This increase is believed to be due to the greater mass of gutta percha used in covering this coil, which was of larger dimensions than the two others.

PART II

Professor Thomson has supplied an equation expressing the law which connects the resistance of a cylindrical covering, such as that of a cable, with the resistance of the unit of the material forming the covering

Let S be the specific resistance of the material, or the resistance of a bar one foot long, and one square foot in section, let G be the resistance of the cylindrical cover of a length of cable L, let $\frac{a}{b}$ be the ratio of the external to the internal diameter of the covering, then

$$S=\frac{2\pi LG}{\log\frac{a}{b}} \qquad (1)$$

The resistance G was calculated from cables of various lengths, lying in iron wells at the works of R S Newall and Co, Birkenhead The cables were not wet, but direct experiment proved that covering a sound iron-covered cable with water has no effect on the loss The details of this experiment are given in the paper

The resistance G was obtained in the following manner The copper conductor of the cable to be tested was arranged so as to form a complete metallic arc with a battery of 72 cells and a tangent galvanometer. the deflection on this galvanometer was read and entered as the continuity test. Deflections were then read on the same galvanometer with the battery and several known resistances in circuit, for the purpose of measuring the resistance and electromotive force of the battery, in the manner described by Pouillet The deflection caused by the loss was next read on a second tangent galvanometer the same battery was used This deflection was entered as the insulation test The temperature of the tank containing the cable was observed by means of a thermometer inserted in a metal tube, extending from the circumference into the mass of the coil

The relative delicacy of the galvanometers was ascertained by experiment, or, in other words, the coefficient was found by which the tangents of the deflections of the first were multiplied to render them directly comparable with the tangents of the deflections of the second galvanometer

The resistances of the galvanometer coils, of the artificial resistance coils, and of the copper conductor of the cable were measured by Wheatstone's differential arrangement. Special experiments were made by means of this differential arrangement to determine the change of resistance of the copper conductor in the cable, produced by a change of temperature

The equation (No 2) $R=r(1+0\ 00192t)$ gives the value of the resistance R of the copper wire at any temperature $t+a$ in function of the resistance r at any temperature a (Fahrenheit) The length and temperature of any coil being known, the resistance of the copper wire was thus at once obtained from the resistance of one knot at 60°, which was very carefully determined

Now let G=resistance of cylindrical coating
D=deflection called the continuity test
d=deflection called the insulation test
C=coefficient expressing the relative delicacy of the two galvanometers.
BR=resistance of the battery
T_1=resistance of the coil of first galvanometer
T_2=resistance of the coil of second galvanometer

$$\text{Then } G=\frac{C\tan D\times(BR+T_1+M)}{\tan d}-BR+T_2+\frac{M}{2}\qquad(3)$$

G having been thus obtained in any desired units, S, the specific resistance of the material, can be at once obtained by equation No 1, which appears from several experiments to give constant values for S when calculated from cables of different dimensions In extreme cases, however, the influence of extra resistance would render the formula defective, especially after continued application of the current · thus the resistance of a foot-cube would be very different to that of an inch-cube

The values of G for the covering of the Red Sea cable, after continued electrification for periods of one, two, three, four, and five minutes, were calculated in Thomson's Absolute British Units, from four sets of tests made specially for this purpose on four different cables, each about 500 knots long. Tables containing the results of these calculations accompany the paper

A Table is also given of the resistance of the Red Sea covering after one minute's electrification, and after five minutes' electrification, at each degree of temperature, from 50° to 75° Fahrenheit. This Table was formed by means of the temperature curves described in the first part of the paper: this Table is here annexed (No 1)

Similar Tables were given for the covering of the two experimental coils mentioned in the first part of the paper. The coil composed of pure gutta percha, gave very regular and complete results. An abbreviation of the Table is annexed

It was remarked that in the tests of the cable in the iron tanks, the resistance after five minutes' electrification was invariably greater with zinc than with copper to cable, whilst the reverse was the case with the single knot covered by water The length of the cable, and the condition of immersion or non-immersion, have probably some influence on the phenomenon of extra-resistance This phenomenon appears to the author to be of much importance, and to demand further investigation

The values of G were also calculated from the daily tests of the cables during manufacture at many temperatures These values agreed with those given in the Tables above described. The general results of the experiments may be summed up as follows

The relative loss at various temperatures through pure gutta percha has been pretty accurately determined for all ordinary temperatures To a less extent the same knowledge has been gained concerning two other coatings containing Chatterton's compound. The latter appears superior at high, and inferior at low temperatures.

Attention has been drawn to the considerably increased resistance which follows the continued electrification of gutta percha and its compounds Some of the laws of this extra resistance have been determined, and some suggestions made as to the cause of the phenomenon

The bounds have been pointed out within which formulæ may be used, which consider gutta percha as a conductor of the same nature as metals

The resistance of gutta percha has been obtained in units, such as are employed to measure the resistance of metals, and by the use of Professor Thomson's formula, the specific resistance of a unit of the material has been fixed with some accuracy

The resistance of other non-conductors, such as glass and the resins, may probably, by comparison with gutta percha, be obtained in the same units.

Incidentally, the increase of resistance in copper with increased temperature has been given from new experiments, and it has been shown that the insulation of a sound wire-covered cable is little, if at all, affected by submersion.

Finally, tables and formulæ are given by which the resistance of, or the loss through any new cable coated with gutta percha, may be at least approximately estimated —

TABLE I

Specific Resistance in Thomson's Units of the Red Sea Covering at various Temperatures

Temperature	Zinc to cable		Copper to cable	
	After electrification for one minute	After electrification for five minutes	After electrification for one minute	After electrification for five minutes
60°	2162×10^{17}	3330×10^{17}	2239×10^{17}	3405×10^{17}
65	$1810 \times$ ”	$2947 \times$ ”	$1720 \times$ ”	$2770 \times$ ”
70	$1460 \times$ ”	$2378 \times$ ”	$1318 \times$ ”	$2239 \times$ ”
75	$1160 \times$ ”	$1753 \times$ ”	$1000 \times$ ”	$1739 \times$ ”

TABLE II

Specific Resistance in Thomson's Units of pure Gutta Percha at various Temperatures

Temperature	Zinc to cable		Copper to cable	
	After electrification for one minute	After electrification for five minutes	After electrification for one minute	After electrification for five minutes
50	4113×10^{17}	5663×10^{17}	4113×10^{17}	5663×10^{17}
55	$2917 \times$ ”	$3636 \times$ ”	$2917 \times$ ”	$3636 \times$ ”
60	$2163 \times$ ”	$2549 \times$ ”	$2163 \times$ ”	$2549 \times$ ”
65	$1634 \times$ ”	$1858 \times$ ”	$1634 \times$ ”	$1858 \times$ ”
70	$1162 \times$ ”	$1291 \times$ ”	$1193 \times$ ”	$1291 \times$ ”
75	$805 \times$ ”	$877 \times$ ”	$796 \times$ ”	$866 \times$ ”
80	$566 \times$ ”	$613 \times$ ”	$548 \times$ ”	$591 \times$ ”

III. "On Scalar and Clinant Algebraical Coordinate Geometry, introducing a new and more general Theory of Analytical Geometry, including the received as a particular case, and explaining 'imaginary points,' 'intersections,' and 'lines.'" By ALEXANDER J. ELLIS, Esq, B A, F C P.S Communicated by ARCHIBALD SMITH, Esq Received February 16, 1860

(Abstract)

Scalar Plane Geometry —With O as a centre describe a circle with a radius equal to the unit of length Let OA, OB be any two of its unit radii, termed 'coordinate axes.' From any point P in the plane AOB draw PM parallel to BO, so as to cut OA, produced either way if necessary, in M Then there will exist some 'scalars' ('real' or 'possible quantities') u, v such that $\mathrm{OM}=u$ OA, and $\mathrm{MP}=v$ OB, all lines being considered in respect both to magnitude and direction Hence OP, which is the 'appense' or 'geometrical sum' of OM and MP, or $=\mathrm{OM}+\mathrm{MP}$, will $=u\ \mathrm{OA}+v\ \mathrm{OB}$ By varying the values of the 'coordinate scalars' u, v, P may be made to assume any position whatever on the plane of AOB. The angle AOB may be taken at pleasure, but greater symmetry is secured by choosing OI and OJ as coordinate axes, where IOJ is a right angle described in the right-handed direction. If any number of lines OP, OQ, OR, &c , be thus represented, the lengths of the lines PQ, QR, &c , and the sines and cosines of the angles IOP, POQ, QOR, &c , can be immediately furnished in terms of the unit of length and the coordinate scalars.

If $\mathrm{OP}=x\,.\,\mathrm{OI}+y\,.\,\mathrm{OJ}$, and any relation be assigned between the values of x and y, such as $y=fx$ or $\phi(x, y)=0$, then the possible positions of P are limited to those in which for any scalar value of x there exists a corresponding scalar value of y The ensemble of all such positions of P constitutes the '*locus*' of the *two* equations, viz the 'concrete equation' $\mathrm{OP}=x\ \mathrm{OI}+y\ \mathrm{OJ}$, and the 'abstract equation' $y=f\ x$. The peculiarity of the present theory consists in the recognition of these *two* equations to a curve, of which the ordinary theory only furnishes the latter, and inefficiently replaces the former by some convention respecting the use of the letters, whereby the coordinates themselves are not made a part of the calculation.

A variation in either of these two equations will occasion a difference either in the form or position of their locus If the abstract equation be $y=ax+b$, where a and b are any scalars, the concrete equation $\mathrm{OP}=x\,.\,\mathrm{OI}+y\,.\mathrm{OJ}$ becomes $\mathrm{OP}=x\,.\,(\mathrm{OI}+a\,.\,\mathrm{OJ})+b\ \mathrm{OJ}$, which shows that OP is the appense of a constant line, and a line in a constant direction, and hence its extremity P must lie on a line in that direction drawn through the extremity of the constant line Also, since the length of OP is $\sqrt{(x^2+y^2)}$ times the length of OI, the locus of the two equations

$$\mathrm{OP}=x\ \mathrm{OI}+y\,.\,\mathrm{OJ} \text{ and } x^2+y^2=a^2$$

must be a circle From these equations all the usual theory of the straight line and circle may be readily deduced, and all ambiguity respecting the representation of direction by the signs (+) and (−) may be removed Thus if the loci of

$$\mathrm{OP} = x\ \mathrm{OI} + y\ \mathrm{OJ}, \quad y = ax + b,$$

and

$$\mathrm{OP}' = x'\ \mathrm{OI} + y'\ \mathrm{OJ}, \quad ay' + x' = 0$$

intersect in the point Q, and OQ_1 be the unit radius on the J side of OI (produced both ways), determined by the equation

$$\sqrt{(1+a^2)}\ \mathrm{OQ}_1 = -a\ \mathrm{OI} + \mathrm{OJ},$$

then $\sqrt{(1+a^2)}\ \mathrm{OQ}$ will $= b\,\mathrm{OQ}_1$ And a line drawn from the locus of P parallel to OQ to pass through the point X, where $\mathrm{OX} = x_1 \,.\, \mathrm{OI} + y_1\ \mathrm{OJ}$, will be represented in magnitude and direction by

$$\frac{y_1 - ax_1 - b}{\sqrt{(1+a^2)}}\ \mathrm{OQ}_1.$$

From this result the usual theory of anharmonic ratios is immediately deducible without any fresh 'convention' respecting the signs (+) and (−).

As every locus has *two* equations, each equation requires separate consideration The investigations concerning the abstract equation remain nearly the same as in the usual theory When the abstract equation is given indirectly by the elimination of two constants between three equations, the result corresponds to the locus of the intersection of two curves varying according to a known law, 'coordinates proper,' leading in its simplest forms, first, to Descartes' original conception of curves generated by the intersection of straight lines moving according to a given law parallel to two given straight lines, and secondly, to Plucker's 'point coordinates' The true relation of Plucker's 'line coordinates' to the ordinary system is immediately apparent on comparing the two sets of equations

(1) Concrete $\mathrm{OP} = x\ \mathrm{OI} + y\ \mathrm{OJ}$
Abstract $F(x, y) = 0,$
(2) First abstract $y = ax + b$
Second abstract $F(a, b) = 0$

The second set of equations determines a curve by supposing a and b to vary, then eliminating a and b, and referring the ultimate abstract equation to the concrete equation (1) On comparing these two sets of equations, we see that x and y in the first are involved in precisely the same manner as a and b in the second, so that if any equation $F(u, v)=0$ were given, it might determine one or the other by precisely the same algebra, according as x, y or a, b were substituted for u, v Whence flow Plucker's theories of collineation and reciprocity

The investigations respecting the concrete equation, on the other hand, are altogether new *The most general form of the two* equations, is

$$\text{Concrete, } OR=f_1(x, y)\ OA+f_2(x, y)\ OB$$
$$\text{Abstract, } \phi(x, y)=0,$$

which will clearly determine a curve as definitely as before. If in lieu of the abstract equation $\phi(x, y)=0$, we were given the locus of the two equations

$$OM=x\ OA+y\ OB,\quad \phi(x, y)=0,$$

and from any point M in this curve we drew MN parallel to OB, cutting OA produced in N, so that $ON=x\ OA$, $NM=y\ OB$, we could find x and y from this curve, and consequently form

$$OL=f_1(x, y)\ OA, \text{ and } LR=f_2(x, y)\ OB,$$

and by this means determine the point R, where $OR=OL+LR$, in the locus *of the general equations, corresponding to the point* M in the particular locus The general form is therefore the algebraical expression of a curve formed from another curve by means of operations performed on the coordinates of the points in the latter, as when an ellipse is formed from a circle by altering the ordinates in a constant ratio.

The algebraical treatment of this case consists in putting

$$p=f_1(x, y),\ q=f_2(x, y),$$

and between these equations and $\phi(x, y)=0$, eliminating x and y, *to find* $\psi(p, q)=0$. The locus is then *reduced to that of*

$$OR=p\,.\,OI+q\,.\,OJ,\ \psi(p, q)=0,$$

which is the ordinary simple case. But the whole of this latter locus does not in all cases correspond to the locus of the general equations, because not only x and y, but also p or $f_1(x, y)$, and q or

$f_2(x, y)$ must be all scalar. Thus, in the case of the parabola derived from a circle, by substituting for the ordinate of a point in the latter its distance from a known point in the circumference of the same, we know that it is impossible to derive more of the parabola than can be obtained by taking the diameter of the circle as the ordinate The algebraical process gives first

$$(1)\quad \mathrm{OR}=x\ \ \mathrm{OI}+\sqrt{(x^2+y^2)}\ \ \mathrm{OJ},\qquad y^2+x^2=2ax,$$

and then, putting $p=x$, $q=+\sqrt{(x^2+y^2)}$, we find

$$(2)\quad \mathrm{OR}=p\ \ \mathrm{OI}+q\,.\,\mathrm{OJ},\ q^2=2ap$$

In this case q is always positive and $=+\sqrt{(2ax)}$. But x, and therefore p, always lies between the limits 0 and $2a$, and hence q must lie between the same limits Consequently the only part of the curve (2) represented by the equations (1) is the semi-parabola contained between the origin and the ordinate $2a$ OJ

This general form of the concrete equation, therefore, furnishes an elementary method of representing curves or parts of curves Thus

$$\mathrm{OR}=(h+a\cos\alpha+x\cos\alpha)\ \ \mathrm{OI}+\big(k(+a\sin\alpha+r\sin\alpha)\big)\ \ \mathrm{OJ}$$
$$a^2+y^2=a^2,$$

are the equations of a line

$$\mathrm{HK}=2a\ (\cos\alpha\ \ \mathrm{OI}+\sin\alpha\ \ \mathrm{OJ}),$$

and having one of its extremities determined by the equation

$$\mathrm{OH}=h\ \ \mathrm{OI}+k\ \ \mathrm{OJ},$$

so that its length is $2a$ times that of OI and the angle (OI, HK)$=\alpha$

To determine the intersections of two such finite curves, given by the equations

$$\mathrm{OP}=f_1(x, y)\,.\,\mathrm{OI}+f_2(x, y)\,.\,\mathrm{OJ},\quad \phi(x, y)=0,$$
and
$$\mathrm{OP}'=f_1'(x', y')\ \ \mathrm{OI}+f_2'(x', y')\ \ \mathrm{OJ},\quad \phi'(x', y')=0,$$

we have OP$=$OP$'$, and consequently $f_1=f_1'$ and $f_2=f_2'$, which, with $\phi=0$ and $\phi'=0$, give four equations to determine x, y, x', y'. The curves will, however, not intersect, unless not only four such scalars exist, but they make f_1, f_2, f_1', f_2' all scalar

The transformation of coordinates may now be investigated more generally in the form of the two problems · 'given a change in the concrete (or abstract) equation, to find the corresponding change in the abstract (or concrete) equation respectively, in order that the locus may remain unaltered' The ordinary theory only comprehends an exceedingly simple instance of the first problem The second is indeterminate so far as the representation of portions of curves is concerned, so that any abstract equation may, by the help of a properly selected concrete equation, represent any curve whatever.

A curve by which the scalar value of y is exhibited corresponding to any scalar value of x in the equation $\phi(x, y)=0$, and which in this simple case is furnished by the locus of the two equations

$$OP=x\ OI+y\ OJ, \quad \phi(x, y)=0,$$

is termed the 'scalar radical locus' of the abstract equation

$$\phi(x, y)=0,$$

and corresponds to what has been hitherto insufficiently designated as the 'locus of the equation' $\phi(x, y)=0$ It presents a necessarily imperfect image of that equation

Clinant Plane Geometry.—Reverting to the original pair of equations

$$OP=x\ OI+y\ OJ, \quad \phi(x, y)=0,$$

and remembering that even if clinant ('impossible' or 'imaginary') values were substituted for x and y in the expressions $x\,.\,OI$, $y\ OJ$, they would still represent definite lines (see abstract of Paper on 'Laws of Operation, &c ,' Proceedings, vol. x p 85), and consequently the line OP would still be perfectly determined, we see that the limitation of x and y to scalar values in the previous investigations was merely a matter of convenience We may therefore give x any clinant value, and after determining the correspondent clinant value of y from $\phi(x, y)=0$ (which will always exist if the equation is algebraical), substitute these values in the concrete equation, and thus find OP, and consequently the locus of the equations. We may observe, however, that as a clinant involves *two* scalars, we must have some relation given between them, directly or indirectly, in order that there may be only one real variable, without which limitation the locus would in every case embrace the whole plane

The general algebraical process is as follows, the Roman letter i being used for $+\sqrt{(-1)}$ Let $OP=P(X_1, X_2 \ldots X_n)\,.\,OI$, where $X_1=p_1+\mathrm{i}\ q_1, \ldots X_n=p_n+\mathrm{i}\,.\,q_n$, and all the p, q are scalar. We can reduce this expression to $OP=(P_1+\imath \cdot P_2) \cdot OI$, where P_1 and P_2 are scalar 'formations' (or 'functions') of the $2n$ scalars $p_1 \ldots p_n$, $q_1 \ldots q_n$. As this is equivalent to $OP=P_1 \cdot OI+P_2 \cdot OJ$, it is precisely the same as the general scalar concrete equation lately investigated. But one abstract equation will now no longer suffice, for if we put $P_1=x$, $P_2=y$, we must have $2n-1$ additional equations, in order ultimately to find $f(x, y)=0$ by eliminating $2n$ variables between $2n+1$ equations. The result, $OP=x \cdot OI+y \cdot OJ$, $f(x, y)=0$, with the conditions of scalarity, will then enable us to determine the locus by the usual process.

If there be only two clinants, $X=p+\imath \cdot q$, and $Y=r+\imath \cdot s$, and we have given $OP=P \cdot (X, Y) \cdot OI$, $C \cdot (X, Y)=0$, where $C=0$ may be termed the 'curve equation,' these reduce to

$$OP=P_1 \cdot OI+P_2 \cdot OJ, \quad C_1=0, \quad C_2=0,$$

where P_1, P_2, C_1, C_2 are formations of p, q, r, s, so that, on putting $P_1=x$, $P_2=y$, we have only four equations, between which we cannot eliminate p, q, r, s. This again shows the necessity of some additional relation, $A(p, q, r, s)=0$, which may be called the 'assignant equation,' in order finally to discover $f(x, y)=0$, and thus determine the locus.

The only case ordinarily considered is where X is scalar. This corresponds to putting $q=0$ for the assignant equation. Hence q disappears and we have

$$OP=P_1 \cdot OI+P_2 \cdot OJ, \quad C_1=0, \quad C_2=0,$$

where P_1, P_2, C_1, C_2 are formations of p, r, s, and hence, putting $P_1=x$, $P_2=y$, we immediately eliminate p, r, s, and determine the locus.

This general theory is illustrated by numerous examples, and in particular Plucker's 'involutions' by means of 'imaginary lines' are fully explained by help of the really existent lines of this theory.

The general theory of the intersection of two 'clinant loci' is precisely analogous to that of two scalar loci with general concrete equations. In the particular case where the concrete equations are the same for both, or the reduced equations are

$$OP=[P_1(p, q, r, s)+\imath \cdot P_2(p, q, r, s)] \cdot OI,$$
$$C_1(p, q, r, s)=0, \; C_2(p, q, r, s)=0, \; A(p, q, r, s)=0,$$

and $OP'=[P_1(p', q', r', s')+\imath \cdot P_2(p', q', r', s')] \cdot OI$,

$$C_1'(p', q', r', s')=0, \; C_2'(p', q', r', s')=0, \; A'(p', q', r', s')=0,$$

the intersection will evidently be determined by putting $p=p'$, $q=q'$, $r=r'$, $s=s'$, which will give six simultaneous equations between four variables, p, q, r, s or p', q', r', s' This gives two equations of condition. If, however, no assignant equations were given, we might determine the values of p, q, r, s from the four reduced curve equations, and then assume any assignant equations compatible with these solutions. This is more readily done by determining X and Y from the two unreduced curve equations $C(X, Y)=0$, $C'(X, Y)=0$ The process then corresponds to that for the simplest scalar case of intersection If the values of X and Y prove to be scalar, then the assignant equations are $q=0$ and $s=0$, and we have an ordinary scalar case of intersection But if this is not the case, and we find

$$X=a_1+\iota.b_1 \qquad a_n+\iota\, b_n,\ Y=c_1+\iota\ d_1 \qquad .\, c_n+\iota\ d_n,$$

we may take

$$(q-b_1)\ \ (q-b_n)=0,\ \ (s-d_1).\ (s-d_n)=0,$$

among others, as assignant equations and determine the corresponding loci These loci will be found to intersect in all the (perfectly real) points determined by the values of X and Y, but not necessarily in these only Such points will of course not belong to the curves derived from putting $q=0$, $s=0$, and hence cannot in any sense be called points of intersection of these curves, although they have hitherto been termed 'imaginary points of intersection'

The discovery of equations to loci described according to some geometrical law, furnishes a convenient illustration of this clinant theory From any point O, draw radii vectores OU, OR to any curves, and make $RP=h$ OU, where h is scalar Put

$$OP=(p+\iota\ q).OI,\ \ OR=(r+\iota.s).OI,\quad OU=(u+\iota\ v).OI.$$

Then the condition $OP=OR+RP$ gives $p=r+hu$, $q=s+hv$, which correspond to the two reduced curve equations. We now require three more equations in order to eliminate four of the six scalars p, q, r, s, u, v, and find a relation between the two remaining scalars, sufficient with one of the above concrete equations to determine the locus of one of the points P, R, U. Two of these three equations will amount to assigning the locus of two of these points, and the third equation will amount to assigning some relation between the angular motions of OU and OR, without which the loci can clearly not be determined This third condition is frequently given in the form of requiring M (a point in RP, produced either way if necessary where $\mathrm{RM}=k\ \mathrm{OU}$ and k is scalar) to lie on a given curve It is then most convenient to introduce two new scalars m and n, so that $\mathrm{OM}=(m+\iota\ n)\ \mathrm{OI}$, when the condition $\mathrm{OM}=\mathrm{OR}+\mathrm{RM}$, gives the two additional equations $m=r+ku$, $n=s+kv$, which with the five others will serve to eliminate six of the eight scalars, m, n, p, q, r, s, u, v, and leave the required abstract equation between the remaining two The eliminations are very simple in a great variety of curves This theory is fully illustrated by examples

The first problem in the transformation of coordinates, 'given an alteration in the concrete and curve equations, to find the corresponding alteration in the assignant equation, so that the curves may remain identical, extent excepted,' is solved thus Given

$$\mathrm{OP}=(\mathrm{P}_1+\iota\,.\,\mathrm{P}_2)\ \mathrm{OI},\quad \mathrm{C}_1=0,\quad \mathrm{C}_2=0,\quad \mathrm{A}=0,$$

for the original curve, and

$$\mathrm{OP}'=(\mathrm{P}_1'+\iota\ \mathrm{P}_2')\ \mathrm{OI},\quad \mathrm{C}_1'=0,\quad \mathrm{C}_2'=0$$

for the new equations, where the unaccented letters are formations of p, q, r, s, and the accented of p', q', r', s' Since $\mathrm{OP}=\mathrm{OP}'$, we have $\mathrm{P}_1=\mathrm{P}_1'$, $\mathrm{P}_2=\mathrm{P}_2'$, between which and $\mathrm{C}_1=0$, $\mathrm{C}_2=0$, $\mathrm{A}=0$ eliminate p, q, r, s and use the final equation, which will only involve p', q', r', s' as the assignant equation $\mathrm{A}'=0$, which is independent of any particular form of the curve equations $\mathrm{C}_1'=0$, $\mathrm{C}_2'=0$ The ordinary case of the transformation of coordinates is a particular case of this The second problem, 'given an alteration in the concrete and assignant equations, to find that in the curve equations,' requires the assignant equation to be put in the form $\phi+\psi=0$, which is possible in an infinite number of ways. Then if the locus be that of

$$\mathrm{OP}=x\ \mathrm{OI}+y\ \mathrm{OJ},\quad \phi(x,y)+\psi(x,y)=0,$$

and we have given

$$\mathrm{OP}=[\mathrm{P}_1(p, q, r, s)+\iota\ \mathrm{P}_2(p, q, r, s)]\,.\,\mathrm{OI},$$
$$\phi'(p, q, r, s)+\psi'(p, q, r, s)=0=\mathrm{A},$$

we put $\phi=\phi'$, $\psi=\psi'$, and hence finding

$$x=\xi(p, q, r, s),\qquad y=\eta(p, q, r, s),$$

we use $\mathrm{P}_1=\xi$, $\mathrm{P}_2=\eta$ as the two curve equations. The third problem, 'given an alteration in the curve and assignant equations, to find that in the concrete equation,' admits of a similar solution Given

$$\mathrm{OP} = x \,.\, \mathrm{OI} + y \;\; \mathrm{OJ}, \quad \phi(x, y) + \psi(x, y) = 0,$$

as the equations to the locus, and also

$$\mathrm{C}_1(p, q, r, s) = 0,\ \mathrm{C}_2(p, q, r, s) = 0,\ \mathrm{A} = \phi'(p, q, r, s) + \psi'(p, q, r, s =)0$$

as the new curve and assignant equations put $\phi = \phi'$, $\psi = \psi'$, and determine $x = \xi(p, q, r, s)$, $y = \eta(p, q, r, s)$, and then use

$$\mathrm{OP} = (\xi + i \;\; \eta) \;\; \mathrm{OI}$$

as the new concrete equation The result is independent of the form of the curve equations The geometrical significance of these transformations is that there are no 'families' of plane curves.

'Clinant radical loci,' or curves which furnish a sensible geometrical picture of the relations of the corresponding clinants which satisfy any abstract equation, may be obtained thus Let the given equation be $\mathrm{C}\,(\mathrm{X}, \mathrm{Y}) = 0$ This may be regarded as two reduced curve equations,

$$\mathrm{C}_1\,(p, q, r, s) = 0, \quad \mathrm{C}_2\,(p, q, r, s) = 0.$$

The values of X are assumed by drawing radii vectores to points in any curve, and the corresponding values of Y have to be pictured. We must therefore have some equation $\mathrm{A} = 0$, which in combination with the other two will give the curve by which X is thus determined Eliminating the scalars, p, q, r, s, two and two, between these three equations, we obtain the following six, of which the first and last, and one of the intermediate ones, are in general only required:

$$f_1\,(p, q) = 0,\ f_2\,(p, r) = 0,\ f_3\,(p, s) = 0,\ f_4\,(q, r) = 0,$$
$$f_5(\,q, s) = 0,\ f_6\,(r, s) = 0$$

We now construct the curves containing the points X, S, Y, as the loci of the equations

$$\mathrm{OX} = \mathrm{OP} + \mathrm{PX} = (p + i \;\; q) \,.\, \mathrm{OI}, \qquad f_1(p, q) = 0,$$
$$\mathrm{OS} = \mathrm{OP} + \mathrm{PS} = (p + i \,.\, s) \,.\, \mathrm{OI}, \qquad f_3\,(p, s) = 0,$$
$$\mathrm{OY} = \mathrm{OR} + \mathrm{RY} = \mathrm{OR} + \mathrm{PS} = (r + i \;\; s) \;\; \mathrm{OI}, f_6\,(r, s) = 0.$$

Set off OP at pleasure on OI (produced both ways), and draw the ordinate PXS, cutting the two first curves in X and S Through S draw SY parallel to OI, cutting the third locus in Y: then if $\mathrm{OX} = \mathrm{X} \;\; \mathrm{OI}$, and $\mathrm{OY} = \mathrm{Y} \,.\, \mathrm{OI}$, X and Y are corresponding solutions of $C(X, Y) = 0$ When only scalar solutions are required, the abstract equations to the first and last curve become $q = 0$, $s = 0$, so that both of these curves coincide with OI produced both ways, and the intermediate or connecting curve is the ordinary scalar radical locus

Scalar Solid Geometry.—The same theories apply with proper modifications The concrete equation

$$OP = x\ OI + y\ OJ + z\ OK,$$

with *one* abstract equation, $f(x, y, z) = 0$, gives a surface for its locus, and with *two* abstract equations,

$$f_1(x, y, z) = 0, \quad f_2(x, y, z) = 0,$$

a curve. From these equations all the ordinary theories are most readily deduced We may also take the concrete equation more generally in the form

$$OP = f_1(x, y, z)\ OI + f_2(x, y, z)\ OJ + f_3(x, y, z)\ OK,$$

the abstract equation being $F(x, y, z) = 0$. We obtain the surface by putting $x_1 = f_1$, $y_1 = f_2$, $z_1 = f_3$, and eliminating x, y, z between these equations and $F = 0$, thus finding $\phi(x_1, y_1, z_1) = 0$ The locus of

$$OP = x_1\ OI + y_1\ OJ + z_1\ OK, \quad \phi(x_1, y_1, z_1) = 0$$

must be limited by the condition that not only x, y, z, but also $f_1(x, y, z)$, $f_2(x, y, z)$, $f_3(x, y, z)$ must all be scalar. If three variable parameters a, b, c are introduced, we require in addition three equations of condition, $F_1 = 0$, $F_2 = 0$, $F_3 = 0$, between x, y, z, a, b, c, in order to eliminate all six and find $\phi(x_1, y_1, z_1) = 0$

Clinant Solid Geometry.—Some precaution is now necessary to indicate the *plane* on which the quadrantal rotation symbolized by ι is to take place This is effected by introducing all three coordinate axes into the concrete equations Let the abstract equation be

$$F(X, Y, Z) = 0$$

Then, supposing

$$X = p + \iota q, \quad Y = r + \iota s, \quad Z = u + \iota v,$$

this equation reduces to $F_1 = 0$, $F_2 = 0$, where F_1 and F_2 are formations of the six scalars p, q, r, s, u, v. We now require *two* assignant equations to determine the curves on which OX and OY are to be taken. These may be given in the form of the single clinant equation $A(X, Y, Z) = 0$, which reduces to the two scalar equations $A_1 = 0$, $A_2 = 0$ Now take $OR = f_1(X, Y, Z)\ OI + f_2(X, Y, Z)\ OJ$ Then, since $\iota\ OI = OJ$, because the assumption of the two axes determines the rotation to be in the plane IOJ, we can reduce this to $OR = R_1 . OI + R_2\ OJ$, where R_1, R_2 are formations of the six scalars

By virtue of the equations $A_1 = 0$, $A_2 = 0$, $F_1 = 0$, $F_2 = 0$, which will give q, s, u, v in terms of p, r, the line OR is perfectly determined by the assumption of p and r Next take

$$\sqrt{(R_1^2 + R_2^2)} . OR_1 = OR,$$

so that OR_1 is a unit radius in the direction OR Put

$$OP = f_3(X, Y, Z) . OR_1 + f_4(X, Y, Z)\ OK,$$

which reduces to $OP = R_3\ OR_1 + P_3\ OK$, where R_3, P_3 are formations of the six scalars, because $\iota\ OR_1 = OK$ on the plane $R_1 OK$. The locus of P will now manifestly be a surface, the concrete equation of which becomes of the usual form on putting for OR_1 its value Determine P_1, P_2 by the equations

$$P_1 . \sqrt{(R_1^2 + R_2^2)} = R_1 R_3, \qquad P_2\ \sqrt{(R_1^2 + R_2^2)} = R_2 R_3,$$

and we find

$$OP = P_1\ OI + P_2 . OJ + P_3\ OK$$

Putting $x = P_1$, $y = P_2$, $z = P_3$, and eliminating the six scalars between these three equations and $A_1 = 0$, $A_2 = 0$, $F_1 = 0$, $F_2 = 0$, the locus becomes that of

$$OP = x\ OI + y . OJ + z . OK, \qquad \phi(x, y, z) = 0,$$

which is limited by the conditions of scalarity

After illustrating this theory by an example, the theory of intersection, when the elimination gives clinant values to determine the points, is discussed The general theory is further illustrated by the determination of equations to loci, leading to very simple and extremely general modes of finding families of surfaces. The problems of the transformation of coordinates and of radical loci are shown to be precisely analogous to those of plane curves.

It will be evident that these investigations merely open out a new field for algebraical geometry, of which it is impossible to foresee the extent.

March 29, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The Right Hon. Lord Belper and Dr W. Bird Herapath were admitted into the Society

The following communications were read —

I "On the Volumetric Relations of Ozone and the Action of the Electrical Discharge on Oxygen and other Gases." By THOMAS ANDREWS, M.D., F R.S, and P. G TAIT, Esq., M A Received February 20, 1860

(Abstract.)

This paper contains the full details of the authors' experiments on the volumetric changes which occur in the formation of ozone From three distinct series of experiments, performed by different methods, they show that when ozone is formed from pure oxygen by the action of the electrical discharge, a condensation takes place, as had already been announced in a former Note published in the 'Proceedings' But the condensation is much greater than the earlier experiments of the authors on the expansion by heat of electrolytic ozone had indicated It is, in fact, so great, that if the allotropic view of the constitution of ozone be correct, the density of that body, as compared with oxygen, would be represented by a number corresponding to the density of a solid or liquid rather than that of a gaseous substance This conclusion follows necessarily from the authors' experiments, unless it be assumed that when ozone comes into contact with such substances as iodine, or a solution of iodide of potassium, one portion of it is changed back into common oxygen, while the remainder enters into combination, and that these portions are so related to one another, that the expansion due to the one is exactly equal to the contraction arising from the other For the details of the experiments and of the methods of investigation employed, reference must be made to the original paper

The second part of the communication is devoted to the action of the silent discharge and of the electrical spark on other gases

Hydrogen and nitrogen undergo no change of volume when exposed to the action of either form of discharge Cyanogen is readily decomposed by the spark, but presents so great a resistance to the passage of electricity, that the action of the silent discharge can scarcely be observed Protoxide of nitrogen is readily attacked by both forms of discharge, with increase of volume and formation of nitrogen and hyponitric acid Deutoxide of nitrogen exhibits the remarkable example of a gas which, under the action either of the silent discharge or of the spark, undergoes, like oxygen, a diminution of volume It also is resolved into nitrogen and hyponitric acid Carbonic oxide has given results of great interest, but the nature of the reaction has been only partially investigated The silent discharge decomposes this gas with production of a substance of a bronze colour on the positive wire The spark acts differently, destroying, as in the case of oxygen, the greater part of the contraction produced by the silent discharge The authors are engaged in the further prosecution of this inquiry

II. "On the Equation of Differences for an Equation of any Order, and in particular for the Equations of the Orders Two, Three, Four, and Five" By ARTHUR CAYLEY, Esq , F.R.S. Received March 2, 1860

(Abstract)

The term *equation of differences*, denotes the equation for the squared differences of the roots of a given equation, the equation of differences afforded a means of determining the number of real roots, and also limits for the real roots of a given numerical equation, and was upon this account long ago sought for by geometers. In the Philosophical Transactions for 1763, Waring gives, but without demonstration or indication of the mode of obtaining it, the equation of differences for an equation of the fifth order wanting the second term. the result was probably obtained by the method of symmetric functions. This method is employed in the 'Meditationes Algebraicæ' (1782), where the equation of differences is given for the equations of the third and fourth orders wanting the second terms; and in p. 85 the before-mentioned result for the equation of the fifth order wanting the second term, is reproduced. The formulæ for obtaining by this method the equation of differences, are fully developed by Lagrange in the 'Traité des Equations Numériques' (1808), and he finds by means of them the equation of differences for the equations of the orders two and three, and for the equation of the fourth order wanting the second term, and in Note III he gives, after Waring, the result for the equation of the fifth order wanting the second term. It occurred to me that the equation of differences could be most easily calculated by the following method. The coefficients of the equation of differences, *quà* functions of the differences of the roots of the given equation, are leading coefficients of covariants, or (to use a shorter expression) they are "Seminvariants"*, that is, each of them is a function of the coefficients which is reduced to zero by one of the two operators which reduce a covariant to zero. In virtue of this property they can be calculated, when their values are known, for the particular case in which one of the coefficients of the given equation is zero. To fix the ideas, let the given equation be $(*\between v, 1)^n=0$; then, when the last coefficient or constant term vanishes, the equation breaks up into $v=0$ and into an equation of the degree $(n-1)$, which I call the reduced equation, the equation of differences will break up into two equations, one of which is the equation of differences for the reduced equation, the other is the equation for the squares of the roots of the same reduced equation. This hardly requires a proof, let the roots of the given equation be $\alpha, \beta, \gamma, \delta$, &c, those of the equation of differences are $(\alpha-\beta)^2$, $(\alpha-\gamma)^2$, $(\alpha-\delta)^2$, &c., $(\beta-\gamma)^4$, $(\beta-\delta)^2$, $(\gamma-\delta)^2$, &c, but in putting the constant term equal to zero, we in effect put one of the roots, say α, equal to zero, the roots of the equation of differences thus become $\beta^2, \gamma^2, \delta^2$, &c, $(\beta-\gamma)^2$, $(\beta-\delta)^2$, $(\gamma-\delta)^2$, &c. The equation for the squares of the roots can be found without the slightest difficulty, hence if the equation of differences for the reduced equation of the order $(n-1)$ is known, we can, by combining it with the equation for the squares of the roots, form the equation of differences for the given equation with the constant term put equal to zero, and thence by the above-mentioned property of the Seminvariancy of the coefficients, find the equation of differences for the given equation. The present memoir shows the application of the process to equations of the orders two, three, four, and five; part of the calculation for the equation of the fifth order was kindly performed for me by the Rev. R. Harley. It is to be noticed that the best course is to apply the method in the first instance to the forms $(a, b, \between v, 1)^n=0$, without numerical coefficients (or, as they may be termed, the *denumerate forms*), and to pass from the results so obtained to those which belong to the forms $(a, b, \between v, 1)^n=0$, or *standard forms*. The equation of differences, for $(\alpha-\beta)^2$, &c., the coefficients of which are seminvariants, naturally leads to the consideration of a more general equation for $(\alpha-\beta)^2\ (x-\gamma y)^2\ (x-\delta y)^2$, &c., the coefficients of which are covariants; and in fact, when, as for equations of the orders two, three, and four, all the covariants are known, such covariant equation can be at once formed from the equation of differences; for equations of the fifth order, however, where the covariants are not calculated beyond a certain degree, only a few of the coefficients of the covariant equation are given. At the conclusion of the memoir, I show how the equation of differences for an equation of the order n can be obtained by the elimination of a single quantity from two equations each of the order $n-1$, and by applying to these two equations the simplification which I have made in Bezout's abridged method of elimination, I exhibit the equation of differences for the given equation of the order n, in a compendious form by means of a determinant; the method just employed is, however, that which is best adapted for the actual development of the equation of differences for the equation of a given order.

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* The term "Seminvariant" seems to me preferable to M. Brioschi's term Peninvariant.

III. "On the Theory of Elliptic Motion." By ARTHUR CAYLEY, Esq., F.R.S. Received March 9, 1860.

The present Note is intended to give an account of the results which, by means of a grant from the Donation Fund of the Royal Society, I have procured to be calculated for me by Messrs. Creedy and Davis, and which are contained in a memoir presented to the Royal Astronomical Society, entitled "Tables of the Developments of Functions in the Theory of Elliptic Motion." The notation employed is:

r, the radius vector,
f, the true anomaly,
a, the mean distance,
e, the excentricity,
g, the mean anomaly,

so that $$\frac{r}{a}, = elqr(e, g),$$
and $$f, = elta(e, g)$$
(read elliptic quotient radius and elliptic true anomaly), are known functions of e, g Moreover x denotes the periodic part of $\frac{r}{a}$, and y the equation of the centre or periodic part of f, so that

$$\frac{r}{a} = 1 + x,$$
$$f = g + y,$$

and x, y are also known functions of e, g

Formulæ for the development in multiple cosines or sines up to the terms in e^7 of

$$(x^0, x^1 \quad x^7)\,{}^{\cos}_{\sin}\, jy,$$

where j is an indeterminate symbol, are given by Leverrier in the 'Annales de l'Observatoire de Paris,' t. i (1855), pp 346–348, and what has been done is the deduction from these of the developments in the like form of various functions of the forms

$$x^m \,{}^{\cos}_{\sin}\, jf, \left(\frac{r}{a}\right)^{\pm m} {}^{\cos}_{\sin}\, jf,$$

where j has given integer values It is to be remarked that a cosine series is in general represented in the form $\Sigma\,[\cos]^i \cos ig$, where i extends from $-\infty$ to $+\infty$, and the coefficients $[\cos]^i$ satisfy the condition $[\cos]^{-i} = -[\cos]^i$, and that a sine series is represented in the form $\Sigma\,[\sin]^i \sin ig$, where i extends from $-\infty$ to $+\infty$, and the coefficients $[\sin]^i$ satisfy the condition $[\sin]^{-i} = -[\sin]^i$ (this implies $[\sin]^0 = 0$). In the case of a pair of corresponding functions, $x^m \cos jf$ and $x^m \sin jf$, or $\left(\frac{r}{a}\right)^{\pm m} \cos jf$ and $\left(\frac{r}{a}\right)^{\pm m} \sin jf$, one of them expanded in the form $\Sigma\,[\cos]^i \cos ig$, and the other in the form $\Sigma\,[\sin]^i \sin ig$, the sums and differences of the corresponding coefficients $[\cos]^i$, $[\sin]^i$ (represented by the notation $[\cos \pm \sin]^i$, and which are obviously such that $[\cos + \sin]^{-i} = [\cos - \sin]^i$, $[\cos \pm \sin]^0 = [\cos]^0$) are for many purposes equally useful with the coefficients $[\cos]^i$, $[\sin]^i$, and they are in the memoir tabulated accordingly, and the several functions tabulated are as follows: viz

$$(x^1, x^2 \quad . \quad x^7) \qquad\qquad , [\cos]$$

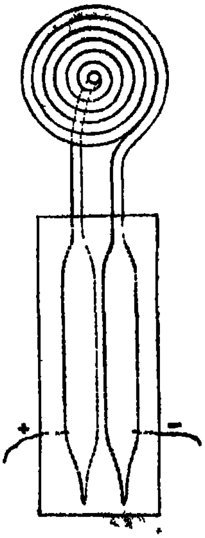
$$(x^0, x^1, x^2 \quad . \quad x^7)\,{}^{\cos}_{\sin} jf,\ j=1 \text{ to } j=7,\ [\cos],\ [\sin],\ [\cos \pm \sin]$$

$$\left(\frac{r}{a}\right)^{+4} \quad .\left(\frac{r}{a}\right)^{+1}, \log\frac{r}{a}, \left(\frac{r}{a}\right)^{-1} . \quad \left(\frac{r}{a}\right)^{-5}\Bigg) \qquad\qquad , [\cos]$$

$$\left(\frac{r}{a}\right)^{+4} \quad \left(\frac{r}{a}\right)^{+1} \quad \left(\frac{r}{a}\right)^{-1} \quad \left(\frac{r}{a}\right)^{-5}\Bigg)\,{}^{\cos}_{\sin} jf,\ j=1 \text{ to } j=5,\ [\cos],\ [\sin],\ [\cos \pm \sin]$$

all the developments being carried up to e^7, the limit of the formulæ from which they are deduced

IV "On the Application of Electrical Discharges from the Induction Coil to the purposes of Illumination" By J. P Gassiot, Esq , F R S Received March 29, 1860.



The subjoined figure represents a carbonic acid vacuum-tube of about $\frac{1}{18}$ of an inch internal diameter, wound in the form of a flattened spiral The wider ends of the tube, in which the platinum wires are sealed, are 2 inches in length and about $\frac{1}{2}$ an inch in diameter, and are shown by the dotted lines, they are enclosed in a wooden case (indicated by the surrounding entire line), so as to permit only the spiral to be exposed.

When the discharge from a Ruhmkorff's induction apparatus is passed through the vacuum-tube, the spiral becomes intensely luminous, exhibiting a brilliant white light Mr. Gassiot, who exhibited the experiment at the meeting of the Society, caused the discharge from the induction coil to pass through two miles of copper wire, with the same coil excited so as to give a spark through air of one inch in length, he ascertained that the luminosity in the spiral was not reduced when the discharge passed through 14 miles of No 32 copper wire.

April 19, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Professor Auguste De la Rive, of Geneva, and Sir John Bowring, were admitted into the Society.

The CROONIAN LECTURE was then delivered by JAMES PETTIGREW, Esq., "On the Arrangement of the Muscular Fibres of the Ventricular portion of the Heart of the Mammal."

(Abstract.)

The Lecturer began by referring to the descriptions of the arrangement of the ventricular fibres of the heart given by previous inquirers, more especially Lower, Senac, Wolff, Gerdy, Duncan, and Reid; he then proceeded to give an account of the results of his own investigations, which had been conducted on the hearts of the Sheep, Calf, Deer, Ox, Horse, &c., all of which, he observed, bear a perfect resemblance to the human heart[*]. In order, as much as possible, to overcome the difficulties of the subject, he availed himself of drawings, explanatory diagrams, and models illustrating the course and relation of the fibres. To these last, however, he observed he attached no special importance, further than that they were useful vehicles of communication; and it was to the dissections themselves, some of which were before the Society, that he looked for a corroboration of the statements he advanced.

Commencing with the left ventricle, which he believes to be the typical one, the Lecturer stated that, by exercising a little care, he had been enabled to unwind as it were its muscular substance, and so to separate its walls into several layers[†], each of which is characterized by a difference in direction. Seven layers, at least, can be readily shown by dissection; but he believes they are in reality nine, viz. four external, the fifth or central, and four internal. He explained how the external fibres are continuous with the internal fibres at the apex, as was known to Lower[‡], Gerdy[§], and others, and how the fibres constituting the several external layers are continuous with corresponding internal layers likewise at the base*,—a fact to which the Lecturer drew particular attention, as being contrary to the generally received opinion, which is to the effect that the fibres at the base are non-continuous, and arise from the auriculo-ventricular tendinous rings—which, as he showed by numerous dissections, is not the case

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[*] Mr. Pettigrew's researches include also the arrangement of the fibres in the ventricles of the Bird, Reptile, and Fish.

[†] Senac (Traité de la Structure du Cœur, &c. [Paris, 1749], planche 8) figures four layers; and Searle (Cyc. of Anat. and Phys., art. "Heart") speaks of three.

[‡] Tractatus de Corde, &c. London, 1669.

[§] Recherches, Discussions et Propositions d'Anatomie, Physiologie, &c. Paris, 1823.

Coming next to the question of the direction of the fibres, he showed how there is a gradational sequence in the direction of the fibres constituting the several layers Thus the fibres of the first layer are more vertical in direction than those of the second, the second than those of the third, the third than those of the fourth, and the fourth than those of the fifth, the fibres constituting which layer are transverse, and run at nearly right angles to those of the first layer Passing the fifth layer, which occupies the centre of the ventricular wall and forms the boundary between the external and internal layers, the order of things is reversed, and the remaining layers, viz six, seven, eight, and nine, gradually return to the vertical in an opposite direction, and in an inverse order This remarkable change in the direction of the external and internal fibres, which had in part been figured by Senac, and imperfectly described by Reid †, as well as other detached and important facts ascertained by himself and others—such as the continuity of the fibres at the apex and base, already adverted to—he suggested might be accounted for by the law of the double conical spiral, which he proceeded forthwith to explain

The expression of the law, as he conceives it, with reference to the arrangement of the fibres in the ventricle, is briefly the following By a simple process of *involution* and *evolution*, the external fibres become *internal* at the apex, and *external* again at the base, so that whether the fibres be traced from without inwards, or from within outwards, they always return to points not wide apart from those from whence they started In order to illustrate the principle of the double conical spiral in the above sense, he took a sheet of net, through which parallel threads of coloured wool, representing the individual fibres, were drawn at intervals, and laying it out on the table before him, with the threads placed horizontally, seized it by the right-hand off corner, and rolled it in upon itself (*i e* towards his own body) seven turns, so as to produce a cone whose walls consisted of nine layers*. On gradually unwinding the walls of the cone thus fashioned (which is tantamount to undoing the spirals), so as to imitate the removal of consecutive layers from the walls of the ventricle, he finds that the gradation in the direction of the several layers just specified is distinctly marked, and that these layers, as was exhibited in various dissections, find a counterpart in the ventricle itself. Thus (the heart being supposed to be placed upright on its apex) in the first external layer the threads are seen running from base to apex, and from left to right †, almost vertically, in the second layer they are slightly oblique, this obliquity increases in the third, and still more in the succeeding layer, till in the fifth or central one the direction of the threads becomes transverse After passing the central layer, the direction of the threads (as of the fibres) is reversed, in the sixth layer they begin to turn from *right* to *left*, with a slight inclination *upwards*, and in succeeding layers gradually become more and more vertical, until the innermost, or ninth, is reached, in which they become as vertical as in the first, but are curved in an opposite direction

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* The late Dr Duncan, Jun, of Edinburgh, was aware of the fibres forming loops at the base, but seems to have had no knowledge of the continuity being occasioned by the union of corresponding *external* and *internal* layers, or that these basal loops were prolongations of like loops formed by similar corresponding external and internal layers at the apex—a point which the Lecturer believes he is the first to establish.

† Cyc of Anat and Phys., art "Heart" London, 1839

As a necessary consequence of this arrangement of the fibres, the Lecturer showed that when the layers are in apposition, as they exist in the undissected ventricle, the first external layer and the last internal cross each other with a slight deviation from the vertical, as in the letter X, while in the succeeding external and internal layers, until the fifth or central one, which is transverse, is reached, they cross at successively wider vertical angles, as may be represented by an ⋈ placed horizontally.

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* A sheet of paper with parallel lines drawn upon it will answer the purpose equally well, except that its non-transparency precludes our seeing the external and internal spirals rolled the one within the other when the sheet is fashioned into a cone and held against the light, as the Lecturer recommends The sheets should be twice as long as they are broad, and the lines or threads should run in the direction of the length

† That is, in the direction from the left hand to the right of the observer.

Holding the cone, prepared as described, against the light, the Lecturer then showed how, by the rolling process, a double system of conical spirals, similar to those found in the left ventricle, had been produced—the one an external left-handed down system, running from base to apex, and corresponding with the external layers, the other an internal right-handed up system, running from apex to base, and corresponding with the internal fibres, and how, seeing the opposite systems are the result of different portions of the same threads being rolled in different directions (the one within the other), the spirals are consequently continuous at the apex.

He in this manner explained the continuity of the external and internal fibres at the apex By simply producing the threads forming the internal spirals, and turning them out at the base until they met corresponding external spirals, he next showed how the continuity of the fibres at the base might be accounted for The connexion of the fibres at the base, he remarked, is effected for the most part as at the apex, by continuity of their proper muscular substance, but those of the papillary muscles are continued by the tendinous cords This continuity observes a certain order, so that certain external layers are continued at the apex into certain internal layers, and turn outwards at the base into their original external position Thus the first layer is continuous with the ninth, the second with the eighth, the third with the seventh, and the fourth with the sixth, while the fifth occupies, as already said, the middle place between the four external and four internal He thus endeavoured to prove that a strong analogy exists between the arrangement of the fibres at the apex and the base, and that the same principle which turns in the external fibres at the apex also turns out the internal at the base,—a view which, while it extends rather than militates against that of older writers, was strongly supported by the arguments he adduced It would therefore seem that the fibres do not form simple loops pointing towards the apex, as generally supposed, but twisted continuous loops pointing alike to apex and base. From this arrangement, it follows that the first and ninth layers embrace in their convolutions those immediately beneath them, while these in turn embrace those next in succession, and so on until the central layer is reached,—an arrangement which may in part explain alike the rolling movements and powerful action of the ventricles

The Lecturer next drew attention to the manner in which the external fibres pass into the interior of the ventricle to form the musculi papillares He first remarked that when the external fibres get into the interior they are necessarily confined to a smaller area, and are therefore crowded into a mass of greater thickness, which contributes to form the papillary muscles He then showed that the external fibres, entering at the apex and forming the "vortex," pass inwards in two principal parcels or bundles, one of which comes chiefly from the posterior surface of the ventricle, and winds forwards to enter the apex anteriorly, whilst another comes from the anterior surface, and winds backwards to enter the apex posteriorly, a fact which the Lecturer believes has been hitherto overlooked On entering the cavity, the anterior bundle crosses to the posterior wall, and forms the posterior papillary muscle, whilst the posterior bundle forms the anterior papillary muscle The fact of this double entrance, and its relation to the papillary muscles, was shown in various preparations, and it was remarked that, but for this double entrance, which applies to all the external layers, the apex of the ventricle would be like the barrel of a pen cut slantingly, or, in fact, lop-sided, whereas, by the arrangement described, it is rendered bilaterally symmetrical.

To bring this bilateral entrance and symmetry into harmony with the description already given of the succession of layers, and with the illustration of the conically rolled sheet, the Lecturer explained that we must regard the primary sheet as having split into two, or we must suppose a second one superadded, and rolled up along with the first. In fact, if a second sheet of net with parallel threads be laid on the first, so that the threads upon it intersect those of the first at an acute angle, and the two are then rolled up together in the way already described, the result will be that the opening at the apex will have two symmetrical lips, as it were, representing the two parcels of fibres forming the vortex in the natural heart.

It is well known that the wall of the left ventricle is thickest at about a third of its length from the base, and that from this point it decreases in thickness towards the base, and still more towards the apex, which is its thinnest part. This condition may be explained by a certain modification of the preceding description,—by supposing, namely (what is really the fact), that the outermost and innermost layers extend further towards the apex and towards the base than those which come next, and these again further than those which succeed, and so on with the rest, the central one being of least extent, and confined indeed to about the middle third of the ventricle. In this way the ventricular wall is thickest towards its middle, where it is composed of all the layers, but becomes thinner and thinner towards the base and apex, where it consists of fewer and fewer layers.

Proceeding next to speak of the right ventricle, and especially of its relation to the left, the Lecturer observed that the simplest way to view that ventricle is to regard it as a segment of the left one, and this view he considers to be most in accordance with what we know of its structure and mode of development For a short time after the heart appears in the embryo, its ventricular compartment is simple; but a septum soon begins to rise up within it, which proceeds from the right side of the apex and anterior wall of the cavity in the direction of the base, and is completed about the eighth week of intra-uterine life. For a time, moreover, the new-formed ventricles have equally thick walls, but as the full period is approached, the left, which is destined after birth to perform a larger amount of work, comes to predominate in thickness. Starting now from the left or "typical" ventricle, constituted as above described, the Lecturer showed that, by pushing in the anterior wall in imitation of the constructive process in the embryo until it reaches the posterior wall, two ventricles are produced, with a partition or septum between. As, however, the septum in this case is double and unattached posteriorly, he said it was necessary, in order to complete the structure, to suppose the fibres forming the posterior border of the septal duplicature as coalescing or anastomosing with corresponding fibres of the posterior wall, whilst the fibres of the two halves of the duplicature itself are blended with each other In this way, as he explained, there results a single septum connected posteriorly, and constituted in a manner which remarkably accords with the structure discovered by dissecting the adult heart. Thus, when both ventricles are dissected at the same time, the fibres forming the external layers posteriorly are found to be for the most part common to both, in other words, the fibres on the back part of the left ventricle cross over the posterior coronary tract, and pass on to the right ventricle, whereas, in front, with the exception of a large cross band at the base, the fibres of the right and of the left ventricle respectively dip inward at the anterior coronary tract, as if altogether independent of each other: an arrangement which induced Winslow to regard the heart as consisting of two muscles enveloped in a third. When, moreover, the so-called common fibres, posteriorly, are dissected layer by layer simultaneously with the independent anterior fibres, it is found that both pass through the same changes of direction, and the same rule holds good with the fibres of the septum.

Another possible mode of explaining the septum, as the Lecturer showed, is to regard the layers entering into the formation of the left ventricle as splitting up posteriorly, the one half of each layer winding round to form the right ventricle, and then dipping in front to form the right half of the septum, whilst the other half proceeds immediately forwards to form the left half of the septum

Both ventricles thus appear to be formed on the same general plan, but they differ materially in the structure of their apices, and the question arises—which is the *primary* or *typical ventricle?* Now, while the fibres of the left ventricle enter its apex in a spiral manner by a species of involution similar to that which would be produced by rolling a sheet of muscle into a cone, those of the right ventricle simply bend or double on themselves Moreover, as the Lecturer suggested, were we to split the septum into two, assigning to each ventricle its proper share, and then apply the cut ends of the common fibres (which cross from the left to the right ventricle posteriorly) to their corresponding fibres in the left half of the septum, we should find that we had still a perfect whole—in other words, a complete system of external and internal spirals, whereas the fibres of the right ventricle and its half of the septum, treated in the same way, would represent only a part of a more complete system—a portion nipped off, as it were, from the side of the perfect cone. Accordingly, if we would dissect the left ventricle, and especially its apex, symmetrically, we must detach the right ventricle as if it were of no account, and dissect layer after layer of the septum *pari passu* with the layers of the left ventricular wall generally, on the other hand, the right ventricle can be dissected only in connexion with the left

For these reasons the Lecturer is inclined to regard the left ventricle as the typical one, and the right as a mere segment thereof, and in further corroboration of this opinion, he referred to the shape of the right and left ventricular cavities, as shown by casts of their interior. The left always yields a beautifully finished and perfect right-handed conical screw, while the cast of the right ventricle, although it has the same twist, represents only an incomplete portion This statement was illustrated by a wax-cast of the ventricles of the heart of a deer

In conclusion, the Lecturer remarked that the arrangement of the fibres composing the ventricles of the mammalian heart, as he had endeavoured to expose it, is characterized by comparative simplicity, and harmonizes perfectly with what is known of the heart's movements

[The matters touched on by the Lecturer are more fully treated of, and the descriptions copiously illustrated by figures, in his Paper entitled "On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Vertebrate Heart" By JAMES PETTIGREW, Esq Communicated by JOHN GOODSIR, Esq , Professor of Anatomy in the University of Edinburgh Received Nov 22, 1859]

April 26, 1860.

Sir BENJAMIN C BRODIE, Bart , President, in the Chair.

The following communications were read —

1. "Note on Regelation." By MICHAEL FARADAY, D.C.L , F R.S. &c Received March 18, 1860.

The philosophy of the phenomenon now understood by the word Regelation is exceedingly interesting, not only because of its relation to glacial action under natural circumstances, as shown by Tyndall and others, but also, and as I think especially, in its bearings upon molecular action; and this is shown, not merely by the desire of different philosophers to assign the true physical principle of action, but also by the great differences between the views which they have taken

Two pieces of thawing ice, if put together, adhere and become one, at a place where liquefaction was proceeding, congelation suddenly occurs The effect will take place in air, or in water, or in vacuo It will occur at every point where the two pieces of ice touch, but not with ice below the freezing-point, i e with dry ice, or ice so cold as to be everywhere in the solid state

Three different views are taken of the nature of this phenomenon When first observed in 1850, I explained it by supposing that a particle of water, which could retain the liquid state whilst touching ice only on one side, could not retain the liquid state if it were touched by ice on both sides, but became solid, the general temperature remaining the same* Professor J Thomson, who discovered that pressure lowered the freezing-point of water†, attributed the regelation to the fact that two pieces of ice could not be made to bear on each other without pressure, and that the pressure, however slight, would cause fusion at the place where the particles touched, accompanied by relief of the pressure and resolidification of the water at the place of contact, in the manner that he has fully explained in a recent communication to the Royal Society‡ Professor Forbes assents to neither of these views, but admitting Person's idea of the gradual liquefaction of ice, and assuming that ice is essentially colder than ice-cold water, i e. the water in contact with it, he concludes that two wet pieces of ice will have the water between them frozen at the place where they come into contact§

Though some might think that Professor Thomson, in his last communication, was trusting to changes of pressure and temperature so inappreciably small as to be not merely imperceptible, but also ineffectual, still he carried his conditions with him into all the cases he referred to, even though some of his assumed pressures were due to capillary attraction, or to the consequent pressure of the atmosphere, only It seemed to me that experiment might be so applied as to advance the investigation of this beautiful point in molecular philosophy to a further degree than has yet been done, even to the extent of exhausting the power of some of the principles assumed in one or more of the three views adopted, and so render our knowledge a little more defined and exact than it is at present.

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* Researches in Chemistry and Physics, 8vo. pp. 373, 378

† Mousson says that a pressure of 13,000 atmospheres lowers the temperature of freezing from 0° to −18° Cent.

‡ Royal Society Proceedings, vol x p 152

§ Proceedings of the Royal Society of Edinburgh, April 19, 1858

In order to exclude all pressure of the particles of ice on each other due to capillary attraction or the atmosphere, I prepared to experiment altogether under water, and for this purpose arranged a bath of that fluid at 32° F A pail, surrounded by dry flannel, was placed in a box, a glass jar, 10 inches deep and 7 inches wide, was placed on a low tripod in the pail, broken ice was packed between the jar and the pail, the jar was filled with ice-cold water to within an inch of the top, a glass dish filled with ice was employed as a cover to it, and the whole enveloped with dry flannel In this way the central jar, with its contents, could be retained at the unchanging temperature of 32° F for a week or more, for a small piece of ice floating in it for that time was not entirely melted away. All that was required to keep the arrangement at the fixed temperature, was to renew the packing ice in the pail from time to time, and also that in the basin cover. A very slow thawing process was going on in the jar the whole time, as was evident by the state of the indicating piece of ice there present.

Pieces of good Wenham-lake ice were prepared, some being blocks three inches square, and nearly an inch thick, others square prisms four or five inches long · the blocks had each a hole made through them with a hot wire near one corner, woollen thread passed through these holes formed loops, which being attached to pieces of lead, enabled me to sink the ice entirely under the surface of the ice-cold water. Each piece was thus moored to a particular place, and, because of its buoyancy, assumed a position of stability The threads were about 1½ inch long, so that a piece of ice, when depressed sideways and then left to itself, rose in the water as far as it could, and into its stable position, with considerable force When, also, a piece was turned round on its loop as a vertical axis, the torsion force tended to make it return in the reverse direction

Two similar blocks of ice were placed in the water with their opposed faces about two inches apart, they could be moved into any desired position by the use of slender rods of wood, without any change of temperature in the water If brought near to each other and then left unrestrained, they separated, returning to their first position with considerable force. If brought into the slightest contact, regelation ensued, the blocks adhered, and remained adherent notwithstanding the force tending to pull them apart They would continue thus, even for twenty-four hours or more, until they were purposely separated, and would appear (by many trials) to have the adhesion increased at the points where they first touched, though at other parts of the contiguous surfaces a feeble thawing and dissecting action went on In this case, except for the first moment and in a very minute degree, there was no pressure either from capillary action or any other cause On the contrary, a tensile force of considerable amount was tending all the time to separate the pieces of ice at their points of adhesion, where still, I believe, the adhesion went on increasing—a belief that will be fully confirmed hereafter

Being desirous of knowing whether anything like soft adhesion occurred, such as would allow slow change of position without separation during the action of the tensile force, I made the following arrangements The blocks of ice being moored by the threads fastened to the lowest corners, stood in the water with one of the diagonals of the large surfaces vertical, before the faces were brought into contact, each block was rotated 45° about a horizontal axis, in opposite directions, so that when put together, they made a compound block, with horizontal upper edges, each half of which tended to be twisted upon, and torn from the other. Yet by placing indicators in holes previously made in the edges of the ice, I could not find that there was the slightest motion of the blocks in relation to each other in the thirty-six hours during which the experiment was continued This result, as far as it goes, is against the necessity of pressure to regelation, or the existence of any condition like that of softness or a shifting contact, and yet I shall be able to show that there is either soft adhesion or an equivalent for it, and from that state draw still further cause against the necessity of pressure to regelation

Torsion force was then employed as an antagonist to regelation. The ice-blocks, being separate, were adjusted in the water so as to be parallel to each other, and about 1½ inch apart If made to approach each other on one side, by revolution in opposite directions on vertical axes, a piece of paper being between to prevent ice contact, the torsion force set up caused them to separate when left to themselves, but if the paper wore away and the ice pieces were brought into contact, by however slight a force, they became one, forming a rigid piece of ice, though the strength was, of course, very small, the point of adhesion and solidification being simply the contact of two convex surfaces of small radius By giving a little motion to the pail, or by moving either piece of ice gently in the water with a slip of wood, it was easy to see that the two pieces were rigidly attached to each other, and it was also found that, allowing time, there was no more tendency to a changing shape here than in the case quoted above If now the slip of wood were introduced between the adhering pieces of ice, and applied so as to aid the torsion force of one of the loops, i e to increase the separating force, but unequally as respects the two pieces, then the congelation at the point of contact would give way, and the pieces of ice would move in relation to each other. Yet they would not separate, the piece unrestrained by the stick would not move off by the torsion of its own thread, though, if the stick were withdrawn, it would move back into its first attached position, pulling the second piece with it, and the two would resume their first associated form, though all the while the torsion of both loops was tending to make the pieces separate.

If when the wood was applied to change the mutual position of the two pieces of ice, without separating them, it were retained for a second undisturbed, then the two pieces of ice became fixed rigidly to each other in their new position, and maintained it when the wood was removed, but under a state of restraint, and when sufficient force was applied, by a slight tap of the wood on the ice to break up the rigidity, the two pieces of ice would rearrange themselves under the torsion force of their respective threads, yet remain united, and, assuming a new position, would, in a second or less, again become rigid, and remain inflexibly conjoined as before.

By managing the continuous motion of one piece of ice, it could be kept associated with the other by a flexible point of attachment for any length of time, could be placed in various angular positions to it, could be made (by retaining it quiescent for a moment) to assume and hold permanently any of these positions when the external force was removed, could be changed from that position into a new one, and, within certain limits, could be made to possess at pleasure, and for any length of time, either a flexible or a rigid attachment to its associated block of ice.

So, regelation includes a flexible adhesion of the particles of ice, and also a rigid adhesion The transition between these two states takes place when there is no external force like pressure tending to bring the particles of ice together, but, on the contrary, a force of torsion is tending to separate them, and, if respect be had to the mere point of contact on the two rounded surfaces where the flexible adhesion is exercised, the force which tends to separate them may be esteemed very great The act of regelation cannot be considered as complete until the junction has become rigid, and therefore I think that the necessity of pressure for it is altogether excluded No external pressure can remain (under the circumstances) after the first rigid contact is broken All the forces which remain tend to separate the pieces of ice, yet the first flexible adhesions and all the successive rigid adhesions which are made to occur, are as much effects of regelation as those which occur under the greatest pressure

The phenomenon of flexible adhesion under tension looks very much like sticking and tenacity, and I think it probable that Professor Forbes will see in it evidence of the truth of his view I cannot, however, consider the fact as bearing such an interpretation, because I think it impossible to keep a mixture of snow and water for hours and days together without the temperature of the mixed mass becoming uniform, which uniformity would be fatal to the explanation. My idea of the flexible and rigid adhesion is this:—Two convex surfaces of ice come together, the particles of water nearest to the place of contact, and therefore within the efficient sphere of action of those particles of ice which are on both sides of them, solidify; if the condition of things be left for a moment, that the heat evolved by the solidification may be conducted away and dispersed, more particles will solidify, and ultimately enough to form a fixed and rigid junction, which will remain until a force sufficiently great to break through it is applied. But if the direction of the force resorted to can be relieved by any hinge-like motion at the point of contact, then I think that the union is broken up among the particles on the opening side of the angle, whilst the particles on the closing side come within the effectual regelation distance; regelation ensues there and the adhesion is maintained, though in an apparently flexible state. The flexibility appears to me to be due to a series of ruptures on one side of the centre of contact, and of adhesion on the other,—the regelation, which is dependent on the vicinity of the ice surfaces, being transferred as the place of efficient vicinity is changed. That the substance we are considering is as brittle as ice, does not make any difficulty to me in respect of the flexible adhesion; for if we suppose that the point of contact exists only at one particle, still the angular motion at that point must bring a second particle into contact (to suffer regelation) before separation could occur at the first; or if, as seems proved by the supervention of the rigid adhesion upon the flexible state, many particles are concerned at once, it is not possible that all these should be broken through by a force applied on one side of the place of adhesion, before particles on the opposite side should have the opportunity of regelation, and so of continuing the adhesion.

It is not necessary for the observation of these phenomena that a carefully-arranged water-vessel should be employed. The difference between the flexible and rigid adhesion may be examined very well in air. For this purpose, two of the bars of ice before spoken of, may be hung up horizontally by threads, which may be adjusted to give by torsion any separating force desired; and when the ends of these bars are brought together, the adhesion of the ice, and the ability of placing these bars at any angle, and causing them to preserve that angle by the rigid adhesion due to regelation, will be rendered evident; and though the flexible adhesion of the ice cannot in this way be examined alone, because of the capillary attraction due to the film of water on the ice, yet that is easily obviated by plunging the pieces into a dish of water at common temperatures, so that they are entirely under the surface, and repeating the observations there. All the important points regarding the flexible and rigid junction of ice due to regelation, can in this way be readily investigated.

It will be understood that, in observing the flexible and rigid state of union, convex surfaces of contact are necessary, so that the contact may be only at one point. If there be several places of contact, apparent rigidity is given to the united mass, though each of the places of contact might be in a flexible and, so to say, adhesive condition It is not at all difficult to arrange a convex surface so that, bearing at two places only on the sides of a depression, it should form a flexible joint in one direction, and a rigid attachment in a direction transverse to the former

It might seem at first sight as if the flexible adhesion of the ice gave us a point to start from in the further investigation of the principle of pressure If the application of pressure causes ice to freeze together, the application of tension might be expected to produce the contrary effect, and so cause liquidity and separation at the flexible joint This, however, does not necessarily follow, nor do I intend to consider what might be supposed to take place whilst theoretically contemplating that case I think the changes of temperature and pressure are too infinitesimal to go for anything, and in illustration of this, will describe the following experiment Wool is known to adhere to ice in the manner, as I believe, of regelation. Some woollen thread was boiled in distilled water, so as thoroughly to wet it. Some clean ice was broken up small and mixed with water, so as to produce a soft mass, and, being put into a glass jar clothed in flannel that it might keep for some hours, had a linear depression made in the surface, so as to form a little ice-ditch filled with water, in this depression some filaments of the wetted wool were placed, which, sinking to the bottom, rested on the ice only with the weight which they would have being immersed in water, yet in the course of two hours these filaments were frozen to the ice In another case, a small loose ball of the same boiled wool, about half an inch in diameter, was put on to a clean piece of ice; that into a glass basin, and the whole wrapped up in flannel and left for twelve hours. At the end of that time it was found that thawing had been going on, and that the wool had melted a hole in the ice, by the heat conducted through it to the ice from the air The hole was filled with the water and wool, but at the bottom some fibres of the wool were frozen to the ice

Is this remarkable property peculiar to water, or is it general to all bodies? In respect of water it certainly seems to offer us a glimpse into the joint physical action of many particles, and into the nature of cohesion in that body when it is changing between the solid and liquid state. I made some experiments on this point Bismuth was melted and kept at a temperature at which both solid and liquid metal could be present, then rods of bismuth were introduced, but when they had acquired the temperature of the mixed mass, no adhesion could be observed between them. By stirring the metal with wood, it was easy to break up the solid part into small crystalline granules, but when these were pressed together by wood under the surface, there was not the slightest tendency to cohere, as hail or snow would cohere in water The same negative result was obtained with the metals tin and lead Melted nitre appeared at times to show traces of the power, but, on the whole, I incline to think the effects observed resulted from the circumstance that the solid rods experimented with had not acquired throughout the fusing temperature Nitre is a body which, like water, expands in solidifying, and it may possess a certain degree of this peculiar power

Glacial acetic acid is not merely without regelating force, but actually presents a contrast to it A bottle containing five or six ounces, which had remained liquid for many months, was at such a temperature that being stirred briskly with a glass rod crystals began to form in it, these went on increasing in size and quantity for eight or ten hours. Yet all that time there was not the slightest trace of adhesion amongst them, even when they were pressed together, and as they came to the surface, the liquid portion tended to withdraw from the faces of the crystals, as if there were a disinclination of the liquid and solid parts to adhere together.

Many salts were tried (without much or any expectation),—crystals of them being brought to bear against each other by torsion force, in their saturated solutions at common temperatures In this way the following bodies were experimented with —Nitrates of lead, potassa, soda; sulphates of soda, magnesia, copper, zinc, alum, borax, chloride of ammonium, ferro-prussiate of potassa, carbonate of soda, acetate of lead; and tartrate of potassa and soda, but the results with all were negative.

My present conclusion therefore is that the property is special for water; and that the view I have taken of its physical cause does not appear to be less likely now than at the beginning of this short investigation, and therefore has not sunk in value among the three explanations given.

Dr Tyndall added to one of his papers*, a note of mine "On ice of irregular fusibility" indicating a cause for the difference observed in this respect in different parts of the same piece of ice The view there taken was strongly confirmed by the effects which occurred in the jar of water at constant temperature described in the beginning of the preceding pages, where, though a thawing process was set up, it was so slow as not to dissolve a cubic inch of ice in six or seven days The blocks retained entirely under water for several days, became so dissected at the surfaces as to develope the mechanical composition of the masses, and to show that they were composed of parallel layers about the tenth of an inch thick, of greater and lesser fusibility, which layers appear, from other modes of examination, to have been horizontal in the ice whilst in the act of formation. They had no relation to the position of the blocks in the water of my experiments, or to the direction of gravity, but had a fixed position in relation to each piece of ice

ADDENDUM, received April 28

The following method of examining the regelation phenomena above described may be acceptable Take a rather large dish of water at common temperatures Prepare some flat cakes or bars of ice, from half an inch to an inch thick, render the edges round, and the upper surface of each piece convex, by holding it against the inside of a warm saucepan cover, or in any other way. When two of these pieces are put into the water they will float, having perfect freedom of motion, and yet only the central part of the upper surface will be above the fluid, when, therefore, the pieces touch at their edges, the width of the water-surface above the place of contact may be two, three, or four inches, and thus the effect of capillary action be entirely removed. By placing a plate of clean dry wax or spermaceti upon the top of a plate of ice, the latter may be entirely submerged, and the tendency to approximation from capillary action converted into a force of separation. When two or more of such floating pieces of ice are brought together by contact at some point under the water, they adhere; first with an apparently flexible, and then with a rigid adhesion. When five or six pieces are grouped in a contorted shape, as an S, and one end piece be moved carefully, all will move with it rigidly, or, if the force be enough to break through the joint, the rupture will be with a crackling noise, but the pieces will still adhere, and in an instant become rigid again. As the adhesion is only by points, the force applied should not be either too powerful or in the manner of a blow I find a piece of paper, a small feather, or a camel-hair brush applied under the water very convenient for the purpose. When the point of a floating, wedge-shaped piece of ice is brought under water against the corner or side of another floating piece, it sticks to it like a leech, if, after a moment, a paper edge be brought down upon the place, a very sensible resistance to the rupture at that place is felt If the ice be replaced by like rounded pieces of wood or glass, touching under water, nothing of this kind occurs, nor any signs of an effect that could by possibility be referred to capillary action, and finally, if two floating pieces of ice have separating forces attached to them, as by threads connecting them and two light pendulums, pulled more or less in opposite directions, then it will be seen with what power the ice is held together at the place of regelation, when the contact there is either in the flexible or rigid condition, by the velocity and force with which the two pieces will separate when the adhesion is properly and entirely overcome

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* Philosophical Transactions, 1858, p 228

II. "Notes on the apparent Universality of a Principle analogous to Regelation, on the Physical Nature of Glass, and on the probable existence of Water in a state corresponding to that of Glass" By Edward W. Brayley, Esq, F.R.S. &c.

Received April 26, 1860

1. Recent experimental investigations, and the reasoning founded upon them, have elevated the designation of an observed property of ice to the character of a principle in physics The growth of crystals of camphor and of iodide of cyanogen, by the deposition of solid matter upon them from an atmosphere unable to deposit like solid matter upon the surrounding glass, except at a lower temperature; and that of crystals in solution, by the deposition of solid matter upon them which is not deposited elsewhere in the solution, have been adduced by Mr. Faraday to illustrate the extension of the principle

of action which is manifested in regelation, and "many such like cases," he remarks, "may be produced" In his reasoning on the nature of that principle, he also rests on the fact, that ice has the same property as camphor, sulphur, phosphorus, metals, &c, which cause the deposition of solid particles upon them from the surrounding fluid, that would not have been so deposited without the presence of the previous solid portions*.

In reflecting on these indications of the universality of the cause, whatever it may intrinsically be, which is operative in the phenomena alluded to, it occurred to me that the known fact of the incorporation of two or more plates of glass into one block, presented a curious parallel to the incorporation of two or more slabs or separate portions of ice into one mass, and to determine in what manner these subjects were related to each other appeared to deserve careful investigation. Towards this the following suggestions are offered.—

Certain substances, both elementary and compound, appear to present, in what we term the solid state, phenomena corresponding to those which are presented by others in the liquid and solid states and the transitions from one to the other collectively regarded, and indicating the existence of a condition of matter which may be termed arrested liquidity, but yet is not, in the most perfect sense, solidity Of these bodies glass is one The fact in question, which exemplifies in a striking manner the property here alluded to, appears to have been first noticed as a subject of scientific importance by MM. Pouillet and Clement Desormes† It is the incorporation, into one mass, of two or more plates of the kind of glass manufactured for mirrors, and called *plate-glass*, the polished surfaces of which have been placed, and have remained for some considerable time, at common temperatures, in close contact with each other, the entire area of one plate being in contact with the entire area of the contiguous one,—extensive mutual surfaces of contact being thus supplied. Under these circumstances, two, three, or four, or even a greater number of plates become converted into one block of glass, which it is impossible to separate into the original plates, and which may be worked, and even cut with a diamond, as if the whole had originally been a single mass In some specimens which I have examined, with the surface of one plate were incorporated portions of another, the surfaces of fracture of which were alone exposed, its substance having been torn through in the effort to separate the united plates by mechanical force* The same effect took place in some experiments by Clement Desormes

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* Exp Res. in Chemistry and Physics, pp 380, 381

† As far as my reading extends, it was first recorded by Pouillet in his 'Elémens de Physique,' liv vi. ch ii 2^{me} édit. Paris, 1832, tome iii p 41 (Bruxelles, 1836, p. 292) In the fourth edition, Paris, 1844, it appears to be omitted, together with other and established facts relating both to glass and to metals.

I assume it to be highly probable that the process by which the two plates of glass become one, is, in reality, analogous to that of regelation in ice, and finally dependent on the same principles, whatever their true character may be conceived or shall ultimately be determined to be To this it may be objected, however, that there is no evidence, in the case of the glass, of the previous liquefaction, or even approach to liquefaction, of the surfaces which become united so as entirely to disappear (or, more properly speaking, to be altogether obliterated), and that the phenomenon is referable simply to the homogeneous attraction of the molecules of one plate for those of the contiguous one, the evenness of the two polished surfaces allowing them to be brought within a very minute distance of one another But two remarkable facts greatly diminish the weight of this objection, if, indeed, they do not entirely remove it First, unpolished plates of glass have no tendency to unite, the hard and compact siliceous film, to which Prof Faraday, regarding glass "as a solution of different substances one in another," long ago referred its power of resisting agents generally †, and which previously bound together the outer molecules of each plate, must be removed by grinding and polishing, so as to render the actual surfaces of contact those of portions of the glass the chemical nature and condition of which are such as readily to admit of their rapid mutual action and union into one mass Secondly, the polished plates sometimes have the forms and configurations of the surfaces of straw and other packing-materials impressed upon them (portions of straw, paper, &c sometimes adhering inseparably to the glass, after having been taken to hot climates*), in consequence of the soft nature of the substance exposed by the polishing, or of its nature being such as readily to soften by a temperature very much below that of the proper fusion, or even softening, of the glass in its integrity. The state of the interior portions of a plate of plate-glass appears, therefore, to be similar to that of glass in general at certain temperatures much below its fusing-point, when it presents such remarkable characters of plasticity, tenacity, and ductility†

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* These and other facts of a similar nature I adduced as illustrative of the physical nature of glass, in lectures on that substance delivered before the Pharmaceutical Society of London in the year 1845. See Pharm. Journ vol v (Oct 1845) pp. 157–160.

† Phil Trans. 1830, Exp Res in Chem and Phys p 282

Is it possible that a lowering of the melting-point of glass, or of the exposed interior portions of it, by pressure, is concerned in the union of the two plates? The effect of the mere pressure of the atmosphere, ensuing upon the exclusion of the air from between the closely apposed plates, would of course be insignificant in depressing the temperature of fusion of the glass, but the pressure occasioned by the cohesive force—exerted, it will be remembered, through a very small thickness only of the material,—which finally unites two or more plates into one block, would probably be adequate to any conceivable effect of this nature which can be required for the production of the phenomenon observed

It may appear at first sight, that the fact that glass belongs to that class of bodies which contract on passing from the liquid to the solid state, and the melting-point of which, therefore, would be elevated—not depressed—by pressure, is opposed to this possibility The objection would be a valid one were we now concerned with glass in a crystalline state. But we are treating of that substance in its familiar and ordinary condition, into which it passes from liquidity by a continuous gradation of temperature, through equally continuous states of softness into the solid form, like melted phosphorus and selenium.

I am now tempted to ask, in conclusion of this part of the subject, Are all cases of the union of two apparently solid surfaces of the same substance by cohesive attraction, cases of melting and regelation, an infinitesimally thin film of liquid being momentarily produced and as instantly solidified? Will two surfaces of perfectly dry ice, at temperatures much below 32°, but under favourable mechanical circumstances, unite by mere apposition and pressure (which ought to follow from Prof James Thomson's theory), and thus prove the identity of the acting principle in the two cases of ice and plate-glass?

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* These particular facts were communicated to me by Mr Tite, F R.S., who had himself observed them

† We are reminded by these facts of the view taken by Person, and adopted by Prof. Forbes, of the similarity of the liquefaction of ice to that of fatty bodies or of the metals, "all which in melting pass through intermediate stages of softness or viscosity," and Sir J F W Herschel (Art "Meteorology," par 119, Enc. Brit. eighth edit), when he terms regelation "a sort of welding," appears to concur in this view.

The negative of the last question does not appear to be proved by the fact cited by Faraday and Tyndall, that dry, hard-frozen snow has not the property of becoming compacted into a snow-ball The cases seem not to be comparable, because the brittleness of the constituent crystals of snow when in this state, its porous nature as a whole, and its being consequently pervaded by air, will prevent the required apposition of surfaces. Nor, as I conceive, is it proved by Prof Tyndall's most instructive experiment of crushing a ball of ice, cooled by carbonic acid and ether, into white and opake hard fragments, for in this also the required apposition of surfaces would be wanting Further, it may be asked, whether this very experiment does not demonstrate the limitation of the lowering of the melting- or freezing-point by pressure? and if so, there can be no tendency to union at 100° below freezing

In discussing the philosophy of the union of two surfaces of glass, I have alluded to the theory of regelation enunciated by Prof J Thomson, but I wish to be understood as not adopting, exclusively, in these notes, any existing theory on the subject Admitting the operation of cohesive attraction and consequent pressure in the first instance, the phenomenon, with respect to glass, readily admits of explanation by the original view of Mr Faraday, which is, "that a film of water must possess the property of freezing when placed between two sets of icy particles, though it will not be affected by a single set of particles" If we regard the two apposed surfaces of glass, each consisting of a thin stratum of particles, taken together, as representing the film of water, then the other strata of particles in contact with them respectively, and making up the entire thickness of the plate on each side, will correspond to the two sets of icy particles, the action of which by freezing the film of water effects the union of the two portions of ice, and the phenomenon may be consistently explained in the terms of Mr Faraday's theory. And here we seem to find points of coincidence between cohesive force, as ordinarily considered, the principle of regelation, and that particular view of the former which has been announced by Mr Faraday in accounting for the phenomena presented by and connected with the latter

2 But we are led by the preceding facts and considerations to some further inferences, if not indeed to a definite hypothesis, upon the subject of the molecular constitution or physical nature of glass Mr Faraday's view of it has been cited already, he regards glass, it will be remembered, "as a solution of different substances one in another." Professor Maskelyne has suggested to me, in conversation, that the physical nature of glass most probably nearly resembles that of a solution of a crystallizable salt in water, immediately before crystallizing These views are evidently coherent, and they harmonize with Prof. Graham's, who defines glass, chemically, as "a mixture of silicates*" But they all relate to the varieties of glass in common use, while we are concerned, at present, with the abstract vitreous condition of matter, such as it is represented by the phosphoric and boracic acids, probably by the heavy optical glass of Faraday, by the simple glasses of felspar and peridote obtained by Charles Deville, by the glassy condition of silica, natural and artificial, and still more perfectly, perhaps, by the glassy form of sugar

Bearing in mind then the homogeneous, or comparatively homogeneous, nature of these glasses, and considering the uniformity of texture which the acoustic as well as the optical characters of perfect glass in general evince, especially when contrasted with that of crystalline plates in the acoustic researches of Savart, and how strongly distinguished that texture is from a crystalline texture or structure,—a nearer analogy than that of a solution ready to crystallize, I think, will be found in the condition of water cooled below the freezing-point but still remaining liquid, until by a tremor, or the percussive contact of a solid body, or the mere contact of a crystal of ice, its temperature rises to 32° and it becomes ice If so, glass will be a substance in which this state of arrested liquidity, or potential solidity, is permanent And this inference will harmonize with known facts Gregory Watt proved that heat is evolved when mineral glasses crystallize or become (permanently and truly) solid * The preparation of sugar called barley-sugar is the vitreous condition of that body, already taken as a type of simple glass, while granular sugar, and more perfectly sugar-candy, exhibit its crystalline state Prof Graham has shown that, at a certain temperature, by mechanical means the former may be converted into the latter, the temperature quickly rising 70° on the transition of the sugar from the glassy to the crystalline state. This and similar facts induced him to refer the peculiar constitution and properties of glass in general to the permanent retention of a certain quantity of heat in a latent state, which becomes sensible on its crystallization, and this will take place on its being preserved in a soft state at certain temperatures.

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* These views of Mr Faraday, Mr. Maskelyne, and Mr Graham are confirmed by the experimental evidence of the structure of glass obtained by Leydolt, to whose researches Professor W H Miller of Cambridge had the kindness to direct me By etching the surface of glass, he found it to have a porphyritic structure, consisting of crystals imbedded in an amorphous substance. But the peculiar characters of glass, especially its relations to sound and light, evince, as indicated in the sequel, that it is not a congeries of ready-formed crystals, though in all probability crystals will always be found on its surface The amorphous substance recognized by Leydolt will answer, nearly, to what I shall call "simple glass" Other facts which he observed are perfectly in harmony with our previous knowledge of the dependence of the texture of glass upon the rate of cooling. See Comptes Rendus, tome xxxiv (1852, April 12) p 565.

There are some remarkable and instructive parallels between the phenomena of the crystallization of water, and that of glass and some other bodies. It follows from the experiments and inductions of Gregory Watt already cited, that during the crystallization of glass a higher temperature must be communicated to the interior than that existing over its surface, by the evolution of heat at the points where the crystalline form is assumed, which will be gradually conducted throughout the mass So that, in the express words of Faraday, in relation to ice, "by virtue of the solidifying [crystallizing] power at points of contact, the same mass may be freezing and thawing at the same moment," and the "freezing process in the inside may be a thawing process on the outside," and thus contribute to the slowness of the cooling, and allow the crystallization therefore to be the more perfect. We here seem to have the explanation

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* Phil Trans 1804, pp 285–290.

of the well-known fact, that in bodies which crystallize from a state of igneous fusion, the most perfect crystalline state is produced when the longest time intervenes between the commencement of solidification (now using that term in its ordinary sense) and the complete cooling of the melted mass The cases cited from Mr Faraday at the beginning of this paper, of the growth of crystals (including those of ice in ice-cold water) in solutions, all have their exact parallels in the accretion of crystals in cooling melted glass "Crystals of ice," Mr Faraday observes, "which could not be colder than the surrounding fluid, exhibited the phenomena of regelation" —that is, of incorporation into one—"when purposely brought in contact with each other" The same thing happens with melted glass slowly cooling, in which crystalline spherules, often forming spontaneously and independently, continue to form and to increase, even after the glass has become solid as such, by the operation of a principle in this view analogous to regelation, until the entire mass has become crystalline*

3 No crystalline body has been longer or more extensively subject to human observation, than crystallized water, or ice Its natural history and properties, as science has advanced, have been investigated with increasing generality and precision, and they have finally become objects of that systematic and exact research which characterizes the present era of physical inquiry,—as is evinced by the dis-

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* If we should prefer to adopt Mr. Maskelyne's suggestion in a formal manner, and regard glass as resembling a solution about to crystallize, its analogue, agreeably to the preceding views, will be a saturated solution of a salt in hot water, allowed to cool undisturbed, and remaining fluid, until its cohesion is affected, when its temperature rises, and the salt crystallizes. Specimens of glass are common which have the aspect and distribution of parts of a crystallized salt in the mother-liquor; opake crystallized spherules appearing in the midst of a transparent mass. To these correspond, among natural glasses, pitchstone and many examples of porphyritic obsidian, consisting of a vitreous base in which crystals have been formed and are imbedded

But at the same time the view I have taken of the subject, and Mr. Maskelyne's, may be equally tenable; for the state of water remaining liquid at temperatures below 32°, and that of saline solutions remaining uncrystallized at temperatures below those of solidification, are evidently closely analogous.

Should I return to this subject, I shall refer to my friend Mr Sorby's observations on the nature of glass, which I had not read when these notes were communicated to the Royal Society, but which are in entire agreement with the views I have suggested —See Quart Journ of Geol Soc vol. xiv. p 465 cussion on regelation, to which these notes are intended to be supplementary A most remarkable deficiency, however, still remains, apparently, in our knowledge of this substance:—*Water in the vitreous condition—Ice-glass—has never been observed.* While we know the antithetical vitreous state of so many different crystallized substances—minerals produced by heat, salts deposited from aqueous solution, neutral bodies of organic origin—and have great reason to believe that that antithetical condition to crystallization is universal, we have no knowledge of it in relation to water or ice. My own attention has been awake to the subject, without success, for many years. It would seem to be scarcely within the bounds of possibility that the glassy state of water, if possessing what we term solidity, should not, ere now, either have been observed in nature, or have occurred and been recognized in experimental research*

I now venture to submit the inquiry, Does this apparent deficiency in our knowledge exist because—to use language recently introduced into physical science—the *homologue* of the glassy state of water is not what we ordinarily term solid—because the state of water cooled below 32° but still liquid is in fact the state which corresponds to the vitreous condition of other bodies, and to the physical nature of perfect ordinary glass? Is the one simply a case of potential solidity, and the other of the confluent or equivalent state of arrested liquidity?

It may be said that the homology which is here endeavoured to be established between liquid water below 32° and glass, is a forced one. That, in relation to each other, these are extreme cases is perfectly true, but intermediate terms of the series are not wanting, and some of them are supplied by sulphur and phosphorus, and in a remarkable manner by selenium All these bodies, when melted, may be

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* The crushed fragments of the ball of ice cooled in carbonic acid and ether, in Prof Tyndall's experiment already mentioned, which "remained *white and opake* as those of crushed glass," were still, he informs me, perfectly crystalline, resembling fragments of quartz.

The "points of analogy between the molecular structure of ice and glass" noticed by Mr Drummond (Phil Mag, August 1859, S 4. vol. xviii. pp. 102–103) do not involve the physical condition of those bodies, but relate merely to the resemblance of one crystallized substance (ice) to another (Reaumur's porcelain), and of both to a third body (bottle- and window-glass), which, from its optical characters, is inferred—I think inconsequentially—to have assumed a state preparatory to crystallization cooled many degrees below their freezing-points and yet remain fluid. Sulphur presents, in its viscid form, an approach to the glassy condition, but it may be obtained in the crystalline form on passing from a state of fusion, and when cooled below freezing, instantaneously crystallizes, like water, by mechanical disturbance

In phosphorus also there is the viscid state, and when cooling after fusion, it passes gradually, like glass, from the liquid to the solid condition without crystallizing, though crystals are deposited from some of its solutions Selenium presents a state resembling the viscid state of the preceding substances, but when melted, and left to cool, remains fluid below its melting-point, and solidifies very gradually in its amorphous state (in which it has some of the characteristic properties of glass), and a thermometer immersed in it during the cooling does not remain stationary at any point, or indicate any temperature at which heat is evolved by molecular change in the substance, —as if the selenium passed continuously from the liquid glassy state to that of solid glass. At ordinary temperatures it retains this condition for a long time—as common glass does at higher, and as water and sulphur will at lower temperatures, but when heated again, between a certain temperature and its melting-point it becomes crystalline and gives out great heat[*] When glass is raised to a certain temperature, and by its maintenence is preserved in a soft state, it does the same

In sulphur, phosphorus, and selenium, therefore, the fluid state below the temperature of solidification—the intermediate condition between fluidity and solidity—the viscid state long retained—the

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[*] These properties of selenium are here stated on the authority of Hittorff, cited in Graham's 'Elements of Chemistry,' second edition, vol ii pp. 688, 689

The case of vanadic acid strongly resembles that of selenium, but extends this *series of concurrent phenomena to a range of temperatures nearly approaching* those which govern the molecular changes of glass It fuses at a red heat, and crystallizes on cooling, but remains fluid below its freezing-point. At the moment solidification commences, it again becomes red-hot, and remains so as long as crystallization continues.

The crystallization of glass, it has been seen, takes place at a high temperature, from the ordinary state of solidity, heat being evolved So the glassy variety of gadolinite (like glass, a silicate with a compound base), when its temperature is elevated above redness, remains solid, but evolves heat (becoming incandescent), and crystallizes, while the crystalline variety merely fuses and intumesces when similarly treated solid state of selenium which evolves heat on crystallizing—all appear to be homologues, at once, of liquid water below 32°, and of the glassy state of matter.

Should this hypothesis be verified, water below 32°, or rather, perhaps, from the temperature of maximum density downwards through that of freezing, may have to be regarded as the type of the vitreous condition of matter, and the causes of the peculiar characters of that condition, its effects on the transmission of the vibrations of sound and light, the conchoidal fracture, &c., may have to be discovered by researches on its molecular nature.

III. "On the Effect of the presence of Metals and Metalloids upon the Electric Conductivity of Pure Copper." By A. MATTHIESSEN, Esq., and M. HOLZMANN, Esq. Communicated by Professor WHEATSTONE. Received March 14, 1860.

(Abstract.)

After studying the effect of suboxide of copper, phosphorus, arsenic, sulphur, carbon, tin, zinc, iron, lead, silver, gold, &c., on the conducting power of pure copper, we have come to the conclusion *that there is no alloy of copper which conducts electricity better than the pure metal.*

May 3, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with the Statutes, the Secretary read the names of the Candidates recommended by the Council for Election into the Society, viz.—

Frederick Augustus Abel, Esq.
Thomas Baring, Esq., M.P.
John Frederic Bateman, Esq.
Edward Brown-Séquard, M.D.
Richard C. Carrington, Esq.
Francis Galton, Esq.
Joseph Henry Gilbert, Esq.
Sir William Jardine, Bart.
Thomas Hewitt Key, Esq.
Joseph Lister, Esq.
Rev. Robert Main, M.A.
Robert William Mylne, Esq.
Roundell Palmer, Esq., Q.C.
John Thomas Quekett, Esq.
Edward Smith, M.D.

The following communications were read:—

I "On the relations between the Elastic Force of Aqueous Vapour, at ordinary temperatures, and its Motive Force in producing Currents of Air in Vertical Tubes." By W D CHOWNE, M D., F R C P. Communicated by JOHN BISHOP, Esq Received March 15, 1860.

(Abstract)

In 1853 the author of this communication made a considerable number of experiments which demonstrated that when a tube, open at both ends, was placed vertically in the undisturbed atmosphere of a closed room, there was an upward movement of the air within the tube of sufficient force to keep an anemometer of light weight in a state of constant revolution, though with a variable velocity An abstract of the results of these experiments was printed in the 'Proceedings' of the Society for June 1855

In order to further investigate the immediate cause or nature of the force which set the machine in motion, the author instituted a series of fresh experiments.

These experiments were made in the room described in the former communication, guarded in the same manner against disturbing causes, and with such extra precautions as will be hereafter explained The apparatus used was a tube 96 inches long and 6 75 inches uniform diameter, the material zinc The upper extremity was open to its full extent, at the lower, the aperture was a lateral one only, into which a piece of zinc tube 3 inches in diameter, and bent once at right angles, was accurately fitted with the outer orifice upward. Within this orifice, which was about 5 inches above the level of the floor, an anemometer, described in the former paper, and weighing 7 grains, was placed in the horizontal position. About midway between the upper and the lower extremity of the tube, a very delicate differential thermometer was firmly and permanently fixed, with one bulb outside and the other inside, and the aperture through which the latter was inserted completely closed The scale was on the stem of the outer bulb

The results of a long series of observations were recorded. The state of the dry and the wet bulb of the hygrometer, as well as the indications of the differential thermometer, was noted, in connexion with the number of revolutions performed per minute by the anemometer. While the differential thermometer indicated the same relative differences between the heat of the atmosphere within and without the tube, the velocity of the revolutions was found to vary considerably This variation was discovered to be chiefly, if not wholly, dependent on the *elasticity* of vapour, due to the hygrometrical state of the atmosphere, as estimated from the dry- and the wet-bulb thermometers, and calculated from the tables of Regnault

240 observations were recorded and afterwards separated into groups, each group comprising those in which the differential thermometer gave the same indication

If in either of these groups we separate into two classes the cases in which the elasticity was highest, from the cases in which it was lowest, and multiply the mean of each with the corresponding mean of the number of the revolutions of the anemometer, their product is nearly a constant, thus showing that the velocity of ascent of the atmospheric vapour is inversely as its elasticity, and hence it follows that the velocity of the ascending current in the tube varies inversely as the density or elastic force of the vapour suspended in the atmosphere This was rendered evident by the aid of Tables appended to the paper

When the mean elastic force of vapour calculated from the dry and the wet bulbs is multiplied by the constant, 13·83, the result gives the whole amount of water in a vertical column of the atmosphere in inches, it follows therefore that when the difference of temperature between the external air and that in the tube, as shown by the differential thermometer, is constant, the velocity of the current in the tube varies inversely as the weight of the vapour suspended in the atmosphere

In an Appendix the author describes some additional experiments, made with the view of ascertaining whether the readings of the differential thermometer were mainly due to actual changes of temperature within the tube, or to extraneous causes acting on the external bulb. He found that when the external bulb was covered with woollen cloth or protected by a zinc tube of about 4 inches diameter and 6 inches long, the temperature of the bulb was increased about 2° on the scale of the instrument, and that when they were removed the prior reading was restored, while the number of revolutions of the anemometer per minute was not appreciably affected by the change This explains why the readings of the differential thermometer varied from 33° 0 to 33° 5 as described in the paper, without producing a corresponding change in the velocity of the anemometer

For the purpose of obtaining a more correct estimate of the influence of a given increase of heat within the tube, the author introduced into the tube at its lowest extremity, a phial containing eight ounces of water at the temperature of 100° Fahr , corked so that no vapour could escape The result showed that in thirteen observations a quantity of heat equal to an increase of one-tenth of a degree on the scale of the differential thermometer, was equivalent to a mean velocity of the anemometer of 3 6 revolutions per minute, the greatest number being 3 8, the least 3 3 per minute

These observations render it still more evident, that if a higher temperature within the tube had been the main cause of the revolutions of the anemometer, the variations in their velocity would not have been in such exact relation to the elastic force of the atmospheric vapour, as has been shown to be the case They also lead to the inference, that the apparent excess of heat within the tube alluded to by the author in his Paper read before the Society in 1855 did not really exist, and to the conclusion that, if such excess had been present, the anemometer would not have been brought to a state of rest by depriving the air of the room of a portion of the moisture ordinarily suspended in it

II "On the Relation between Boiling-point and Composition in Organic Compounds." By HERMANN KOPP, Esq Communicated by Dr. HOFMANN. Received March 20, 1860

(Abstract.)

The author was the first to observe (in 1841) that, on comparing pairs of analogous organic compounds, the same difference in boiling-point corresponds frequently to the same difference in composition This relation between boiling-point and composition, when first pointed out, was repeatedly denied, but is now generally admitted The continued experiments of the author, as well as of numerous other inquirers, have since fixed many boiling-points which had hitherto remained undetermined, and corrected such as had been inaccurately observed In the present paper the author has collected his experimental determinations, and has given a survey of all the facts satisfactorily established up to the present moment regarding the relations between boiling-point and composition

The several propositions previously announced by the author were ,—

1 An alcohol, $C_nH_{n+2}O_2$, differing in composition from ethylic alcohol ($C_4H_6O_2$, boiling at 78° C) by $x\,C_2H_2$, more or less, boils $x \times 19°$ higher or lower than ethylic alcohol

2 The boiling-point of an acid, $C_nH_nO_4$, is 40° higher than that of the corresponding alcohol, $C_nH_{n+2}O_2$

3 The boiling-point of a compound ether is 82° higher than the boiling-point of the isomeric acid, $C_nH_nO_4$

These propositions supply the means of calculating the boiling-points of all alcohols, $C_nH_{n+2}O_2$, of all acids, $C_nH_nO_4$, of all compound ethers, $C_nH_nO_4$ The author contrasts the values thus calculated for these substances with the available results of direct observation The Table embraces eight alcohols, $C_nH_{n+2}O_2$, nine acids, $C_nH_nO_4$, and twenty-three compound ethers, $C_nH_nO_4$, the calculated boiling-points agree, as a general rule, with those obtained by experiment, as well as two boiling-points of one and the same substance determined by different observers We are thus justified in assuming that the calculated boiling-point of other alcohols, acids, and ethers belonging to this series will also be found to coincide with the results of observation

The boiling points of other monatomic alcohols, $C_nH_mO_2$, other monatomic acids, $C_nH_mO_4$, and other compound ethers, $C_nH_mO_4$, are closely allied with the series previously discussed A substance containing xC more or less than the analogous term of the previous class, in which the same number of oxygen and of hydrogen equivalents is present, boils $x \times 14° 5$ higher or lower , or, what amounts to the same thing, a difference of xH more or less of hydrogen lowers or raises the boiling-point by $x \times 5°$ Thus benzoic acid, $C_{14}H_6O_4$, boils 8 × 14° 5 higher than propionic acid, $C_6H_6O_4$, or 8 × 5° higher than œnanthylic acid, $C_{14}H_{14}O_4$, cinnamate of ethyl, $C_{22}H_{12}O_4$, boils 10 × 14° 5 higher than butyrate of ethyl, $C_{12}H_{12}O_4$, or 10 × 5° higher than pelargonate of ethyl, $C_{22}H_{22}O_4$

The author compares the boiling-points thus calculated for five alcohols, $C_nH_mO_4$, for six acids, $C_nH_mO_4$, and for sixteen compound ethers, $C_nH_mO_4$, with the results of observation In almost all cases the concordance is sufficient

The author demonstrates in the next place that in many series of compounds other than those hitherto considered, the elementary difference, xC_2H_2, likewise involves a difference of $x \times 19°$ in the boiling-point He further shows that on comparing the boiling-points of the corresponding terms in the several series of homologous substances hitherto considered, many other constant differences in boiling-point are found to correspond to certain differences in composition Thus a monobasic acid is found to boil 14° higher than its ethyl compound, and 63° higher than its methyl compound, and this constant relation holds good even for acids other than those previously examined, *e g* for the substitution-products of acetic acid Also in substances which are not acids, the substitution of C_4H_5 or C_2H_3 for H, occasionally involves a depression of the boiling-points respectively of 14° and 63°, the relation, however, is by no means generally observed

The author, in addition to the examples previously quoted, shows that compounds containing benzoyl ($C_{14}H_5O_2$) and benzyl ($C_{14}H_7$) boil 78° ($=4 \times 14°\ 5 + 4 \times 5°$) higher than the corresponding terms containing valeryl ($C_{10}H_9O_2$) and amyl ($C_{10}H_{11}$), a relation, however, which is likewise not generally met with He discusses, moreover, other coincidences and differences of boiling-points of compounds differing in a like manner in composition Not in all homologous series does the elementary difference xC_2H_2 involve a difference of $x \times 19°$ in boiling-point. The author shows that this difference is greater for the hydrocarbons, C_nH_{n-6} and C_nH_{n+2}, for the acetones and aldehydes, $C_nH_nO_2$, for the so-called simple and mixed ethers, $C_nH_{n+2}O_2$, for the chlorides, bromides, and iodides of the alcohol radicals, C_nH_{n+1}, and for several other groups, that it is, on the contrary, smaller for the anhydrides of monobasic acids, $C_nH_{n-2}O_6$, for the ethers, $C_nH_{n-2}O_8$ (which may be formed either by the action of one molecule of a dibasic acid, $C_nH_{n\ 2}O_8$, upon two molecules of a monatomic alcohol, $C_nH_{n+2}O_2$, or by the action of two molecules of a monobasic acid, $C_nH_nO_4$, upon one molecule of a diatomic alcohol, $C_nH_{n+2}O_4$), and several other series

The author thinks that the unequal differences in boiling-points corresponding in different homologous series to the elementary difference xC_2H_2, are probably regulated by a more general law, which will be found when the boiling points of many substances shall have been determined under pressures differing from those of the atmosphere

"From the observations at present at our disposal it may be affirmed as a general rule, that in homologous compounds belonging to the same series, the differences in boiling-points are proportional to the differences in the formulæ Exceptions obtain only in cases when terms of a particular group are rather difficult to prepare, or when the substances boil at a very high temperature, at which the observations now at our command are for the most part uncertain Again, it may be affirmed that the difference in boiling-points, corresponding to the elementary difference C_2H_2, is in a great many series $= 19°$, in some series greater, in some series less"

The author proceeds to discuss the boiling-points of isomeric compounds He shows that in a great many cases isomeric compounds belonging to the same type, and exhibiting the same chemical character, boil at the same temperature, and that there is no reason why, for the class of bodies mentioned, this coincidence should not obtain generally On the other hand, different boiling-points are observed in isomeric compounds possessing a different chemical character, although belonging to the same type (*e g* acids and compound ethers, $C_nH_nO_4$, alcohols and ethers, $C_nH_{n+2}O_2$), and in isomeric compounds belonging to different types (*e g* allylic alcohol and acetone).

The author shows that the determination of the boiling-point of a substance, together with an inquiry into the compounds serially allied with it by their boiling-points, constitutes a valuable means of fixing the character of the substance, the type to which it belongs, and the series of homologous bodies of which it is a term He quotes as an illustration eugenic acid The boiling-point of this acid, $C_{20}H_{12}O_4$, is 150°, and on comparing this boiling-point with the boiling-points of benzoic acid, $C_{14}H_6O_4$ (boiling-point 253°), and of hydride of salicyl, $C_{14}H_6O_4$ (boiling-point 196°), it is obvious that eugenic acid cannot be homologous to benzoic acid, whilst, on the other hand, it becomes extremely probable that it is homologous to hydride of salicyl, and consequently that it belongs rather to the aldehydes than to the acids proper.

The author, in conclusion, calls attention to the importance of considering the chemical character in comparing the boiling-points of the volatile organic bases, and shows the necessity of distinguishing between the primary, secondary, and tertiary monamines in order to exhibit constant differences of boiling-point for this class of substances He discusses the boiling-points of the several bases, $C_nH_{n-5}N$ and $C_nH_{n+3}N$, and points out how in many cases the particular class to which a base belongs may be ascertained by the determination of the boiling-point

The comprehensive recognition of definite relations between composition and boiling-point is for the present chiefly limited to organic compounds But for the majority of these compounds, and indeed for the most important ones, this relation assumes the form of a simple law, which, more especially for the monatomic alcohols, $C_nH_mO_2$, for the monobasic acids, $C_nH_mO_4$, and for the compound ethers generated by the union of the two previous classes, is proved in the most general manner, so much so, indeed, that in many cases the determination of the boiling-point furnishes most material assistance in fixing the true position and character of a compound

The author points out more especially that the simplest and most comprehensive relations have been recognized for those classes of organic compounds which have been longest known and most accurately investigated, *and that even for those classes* the generality and simplicity of the relation, on account of numerous boiling-points incorrectly observed at an earlier date, appeared in the commencement doubtful, and could be more fully acknowledged only after a considerable number of new determinations Thus he considers himself justified in hoping that also in other classes of compounds, in which simple and comprehensive relations have not hitherto been traced, these relations will become perceptible as soon as the verification of the boiling-points of terms already known, and the examination of new terms, shall have laid a broader foundation for our conclusions.

III Extract of a Letter from Captain BLAKISTON, R A, to General SABINE, R A, Treasurer and V P R S, dated Singapore, February 22, 1860, giving an account of a remarkable Ice Shower Communicated by General SABINE Received April 19, 1860

"On the 14th January, 1860, when two days out from the Cape of Good Hope, about three hundred miles S S E of it, in lat 38° 53′ S, long 20° 45′ E, we encountered a heavy squall with rain at 10 A M, lasting one hour, the wind shifting suddenly from east to north (true) During the squall there were three vivid flashes of lightning, one of which was very close to the ship, and, at the same time, a *shower of ice* fell which lasted about three minutes. It was not hail, but irregular-shaped pieces of solid ice, of different dimensions, up to the size of half a brick The squall was so heavy that the topsails were let go

"There appears to have been no previous indication of this squall, for the barometer at 6 P M on the two previous days had been at 30 00, therm 70°, at 8 A M on the 14th, 29 82, therm 70°, at 10 A M. (time of squall), 29 86, therm 70°, and at 1 P M, when the weather had cleared, wind north (true), 29 76, therm 69°, after which it fell slowly and steadily during the remainder of the day and following night*

"As to the size of the pieces of ice which fell, two which were weighed, after having melted considerably, were 3½ and 5 ounces respectively, while I had one piece given me, a good quarter of an hour after the squall, which would only just go into an ordinary tumbler. And one or two persons depose to having seen pieces the size of a brick

"On examination of the ship's sails afterwards, they were found to be perforated in numerous places with small holes. A very thick glass cover to one of the compasses was broken

"Although several persons were struck, and some knocked down on the deck, fortunately no one was seriously injured"

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* The weather on the morning preceding the squall was clouded, with close and thick atmosphere, wind E (true), 3 By night of the 14th the wind had hauled to N.W (true), 4, and the day following was W S.W (true), 5—6, cloudy

May 10*th*, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following Gentlemen were proposed by the Council for Election as Foreign Members, and it was announced that they would be balloted for at the ensuing Meeting of the Society, viz —

Alexander Dallas Bache
Hermann Helmholtz
Albert Kolliker
Philippe Edouard Poulletier de Verneuil

The Bakerian Lecture was then delivered by Mr Fairbairn, F R S

The Lecturer gave a condensed exposition of the experiments and results detailed in the following Paper He also exhibited the apparatus employed, and explained the methods followed

"Experimental Researches to determine the Density of Steam at all Temperatures, and to determine the Law of Expansion of Superheated Steam." By WILLIAM FAIRBAIRN, Esq, F R S, and THOMAS TATE, Esq

(Abstract)

The object of these researches is to determine by direct experiment the law of the density and expansion of steam at all temperatures Dumas determined the density of steam at 212° Fahr, but at this temperature only Gay-Lussac and other physicists have deduced the density at other temperatures by a theoretical formula true for a perfect gas

$$\frac{VP}{V_1P_1}=\frac{459+T}{459+T_1} \quad . \quad . \quad (1.)$$

On the expansion of superheated steam, the only experiments are those of Mr. Siemens, which give a rate of expansion extremely high, and physicists have in this case also generally assumed the rate of expansion of a perfect gas. Experimentalists have for some time questioned the truth of these gaseous formulæ in the case of condensable vapours, and have proposed new formulæ derived from the dynamic theory of heat, but up to the present time no *reliable direct*

experiments have been made to determine either of the points at issue. The authors have sought to supply the want of data on these questions by researches on the density of steam upon a new and original method

The general features of this method consist in vaporizing a known weight of water in a globe of about 70 cubic inches capacity, and devoid of air, and observing by means of a "*saturation gauge*" the exact temperature at which the whole of the water is converted into steam The saturation gauge, in which the novelty of the experiment consists, is essentially a double mercury column balanced upon one side by the pressure of the steam produced from the weighed portion of water, and on the other by constantly saturated steam of the same temperature Hence when heat is applied the mercury columns remain at the same level up to the point at which the weighed portion of water is wholly vaporized, from this point the columns indicate, by a difference of level, that the steam in the globe is superheating, for superheated steam increases in pressure at a far lower rate than saturated steam for equal increments of temperature. By continuing the process, and carefully measuring the difference of level of the columns, data are obtained for estimating the rate of expansion of superheated steam

The apparatus for experiments at pressures of from 15 to 70 lbs per square inch, consisted chiefly of a glass globe for the reception of the weighed portion of water, drawn out into a tube about 32 inches long The globe was enclosed in a copper boiler, forming a steam-bath by which it could be uniformly heated The copper steam-bath was prolonged downwards by a glass tube enclosing the globe stem. To heat this tube uniformly with the steam-bath, an outer oil-bath of blown glass was employed, heated like the copper bath by gas jets The temperatures were observed by thermometers exposed naked in the steam, but corrected for pressure The two mercury columns forming the saturation gauge were formed in the globe stem, and between this and the outer glass tube, so long as the steam in the glass globe continued in a state of saturation, the inner column in the globe stem remained stationary, at nearly the same level as that in the outer tube. But when, in raising the temperature, the whole of the water in the globe had been evaporated and the steam had become superheated, the pressure no longer balanced that in the outer steam-bath, and, in consequence, the column in the globe stem rose, and that in the outer tube fell, the difference of level forming a measure of the expansion of the steam Observations of the levels of the columns were made by means of a cathetometer at different temperatures, up to 10° or 20° above the saturation point, and the maximum temperature of saturation was, for reasons developed by the experiments, deduced from a point at which the steam was decidedly superheated

The results of the experiments, which in the paper are given in detail, show that the density of saturated steam at all temperatures, above as well as below 212°, is invariably greater than that derived from the gaseous laws

The apparatus for the experiments at pressures below that of the atmosphere was considerably modified, and the condition of the steam was determined by comparing the column which it supported with that of a barometer The results of these experiments, reduced in the same way, are extremely consistent

As the authors propose to extend their experiments to steam of a very high pressure, and to institute a distinct series on the law of expansion of superheated steam, they have not at present given any elaborate generalizations of their results The following formulæ, however, represent the relations of specific volume and pressure of saturated steam, as determined in their experiments, with much exactness

Let V be the specific volume of saturated steam, at the pressure P, measured by a column of mercury in inches, then

$$V = 25\ 62 + \frac{49513}{P + \ 72} \qquad (2)$$

$$P = \frac{49513}{V - 25\ 62} - 0\ 72 \qquad (3.)$$

In regard to the rate of expansion of superheated steam, the experiments distinctly show that, for temperatures within about ten degrees of the saturation point, the rate of expansion greatly exceeds that of air, whereas at higher temperatures the rate of expansion approaches very near that of air. Thus in experiment 6, in which the maximum temperature of saturation is 174° 92, tho coefficient of expansion between 174° 92 and 180° is $\frac{1}{160}$, or three times that of air, whereas between 180° and 200° the coefficient is very nearly the same as that of air (steam $=\frac{1}{537}$, air $=\frac{1}{549}$), and so on in other cases The mean coefficient of expansion at zero of temperature from seven experiments below the pressure of the atmosphere, and calculated from a point several degrees above that of saturation, is $\frac{1}{498}$, whereas for air it is $\frac{1}{459}$ Hence it would appear that for some degrees above the saturation point the steam is not decidedly in an aeriform state, or, in other words, that it is watery, containing floating vesicles of unvaporized water

Table of Results, showing the relation of density, pressure, and temperature of saturated steam

Number of Exper	Pressure		Max temp of saturation, Fahr	Specific Volume		Proportional error of formula (2)
	in lbs per sq in	in inches of mercury		From experiment	By formula (2)	
1	2 6	5 35	136° 77	8266	8183	$+\frac{1}{100}$
2	4 3	8 62	155 33	5326	5326	0
3	4 7	9 45	159 36	4914	4900	$-\frac{1}{350}$
4	6 2	12 47	170 92	3717	3766	$+\frac{1}{74}$
5	6 3	12 61	171 48	3710	3740	$+\frac{1}{125}$
6	6 8	13 62	174 92	3433	3478	$+\frac{1}{76}$
7	8 0	16 01	182 30	3046	2985	$-\frac{1}{50}$
8	9 1	18 36	188 30	2620	2620	0
9	11 3	22 88	198 78	2146	2124	$-\frac{1}{97}$
1'	26 5	53 61	242 90	941	937	$-\frac{1}{235}$
2'	27 4	55 52	244 82	906	906	0
3'	27 6	55 89	245 22	891	900	$+\frac{1}{100}$
4'	33 1	66 84	255 50	758	758	0
5'	37 8	76 20	263 14	648	669	$+\frac{1}{31}$
6'	40 3	81 53	267 21	634	628	$-\frac{1}{106}$
7'	41 7	84 20	269 20	604	608	$+\frac{1}{150}$
8'	45 7	92 23	271 76	583	562	$-\frac{1}{28}$
9'	49 4	99 60	279 42	514	519	$+\frac{1}{103}$
11'	51 7	104 54	282 58	496	496	0
12'	55 9	112 78	287 25	457	461	$+\frac{1}{114}$
13'	60·6	122 25	292 53	432	428	$-\frac{1}{108}$
14'	56 7	114 25	288 25	448	456	$+\frac{1}{56}$

Adopting the notation previously employed, and putting r for the rate or coefficient of expansion of an elastic fluid at t_1 temperature, we find

$$r=\frac{1}{\epsilon_1+t_1}=\frac{\frac{V_2 p_2}{V_1 p_1}-1}{t_2-t_1}, \quad \ldots \quad (4.)$$

where $\frac{1}{\epsilon_1}$ = the rate of expansion at zero of temperature In the case of air $\epsilon_1 = 459$

The following Table gives the value of the coefficient of expansion of superheated steam taken at different intervals of temperature from the maximum temperature of saturation

Number of the Exper	Max temp of saturation	Temperatures between which the expansion is taken		Coefficient of expansion of superheated steam	Coefficient of expansion of air
1	136°77	140°	170°	$\frac{1}{597}$	$\frac{1}{599}$
2	155 33	160	180	[illegible]	$\frac{1}{619}$
3	159 36	159 36	170 2	[illegible]	$\frac{1}{618}$
		170 2	209 9	[illegible]	$\frac{1}{629}$
5	171 48	171 48	180	[illegible]	$\frac{1}{630}$
		180	200	[illegible]	$\frac{1}{639}$
6	174 92	174 92	180	[illegible]	$\frac{1}{634}$
		180	200	[illegible]	$\frac{1}{639}$
7	182 30	182 3	186	[illegible]	$\frac{1}{641}$
		186	209 5	[illegible]	$\frac{1}{645}$
8	188 30	191	211	[illegible]	$\frac{1}{650}$
1'	242 9	243	249	[illegible]	$\frac{1}{702}$
4'	255 5	257	259	[illegible]	$\frac{1}{716}$
		257	264	[illegible]	$\frac{1}{716}$
6	267·21	268	271	[illegible]	$\frac{1}{727}$
		271	279	[illegible]	$\frac{1}{730}$
7'	269 2	271	273	[illegible]	$\frac{1}{730}$
		273	279	[illegible]	$\frac{1}{732}$
9'	279 42	283	285	[illegible]	$\frac{1}{742}$
		285	289	[illegible]	$\frac{1}{744}$
13'	292 53	297	299	[illegible]	$\frac{1}{756}$
		299	302	[illegible]	$\frac{1}{758}$

Hence it appears, that as the steam becomes more and more superheated, the coefficient of expansion approaches that of a perfect gas The authors hope that these experiments may be continued, and that the results obtained at greatly increased pressures will prove as important as those already arrived at

May 24, 1860

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with Notice given at the last Meeting, the Right Hon Earl de Grey and Ripon was proposed for election and immediate ballot; and the ballot having been taken, his Lordship was declared duly elected

Alexander Dallas Bache, Hermann Helmholtz, Albert Kölliker, and Philippe Edouard Poulletier de Verneuil were severally balloted for, and declared duly elected Foreign Members of the Society.

The following communications were read:—

I. “On a new Method of Approximation applicable to Elliptic and Ultra-elliptic Functions.” By C. W. Merrifield, Esq. Communicated by the Rev. H. Moseley, F.R.S. Received March 26, 1860.

(Abstract.)

I found my method on the known principle, that the geometric mean between two quantities is also a geometric mean between the arithmetic and harmonic means of those quantities.

We may therefore approximate to the geometric mean of two quantities in this way:—Take their arithmetic and harmonic means; then take the arithmetic and harmonic means of those means; then of these last means again, and so on, as far as we please. If the ratio of the original quantities lies within the ratio of 1 : 2, the approximation proceeds with extraordinary rapidity, so that, in obtaining a fraction nearly equal to $\sqrt{2}$ by this method, we obtain a result true to eleven places of decimals at the fourth mean. I name this merely to show the rate of approximation. The real application of the method is to the integration of functions embracing a radical of the square root.

Suppose we wish to approximate to the integral of a function of the form $X\sqrt{Y}$. The function is a geometrical mean between X and XY. If, therefore, we obtain arithmetic and harmonic means to X and XY, and again to these means, and so on, it is clear that our function $X\sqrt{Y}$ will always lie between each pair of means of the series, the arithmetic mean being always in excess, and the harmonic always in defect. I now observe,—

(1) That if the functions X and XY both increase or both decrease regularly with the independent variable, the integral of their geometric mean will always be intermediate to their integrals, and also to each pair of the integrals of the derived means.

(2) That the derived series of arithmetic and harmonic means contain no radicals, and are therefore integrable by resolution into partial fractions, and that their integrals involve only logarithms or inverse tangents.

The last remark indicates that the method has no useful application to functions of a simpler class than elliptic functions. It applies, however, to all elliptic and ultra elliptic functions, and to transcendental functions under a radical The radical must, however, not be higher than the square root, for, although it be true that if we take the case of inserting two means between two quantities, the geometric will still lie between the arithmetic and harmonic means, we have nothing to show what the second step of approximation is to be

The third arithmetic mean is, in the case of elliptic integrals, sufficiently near for working with seven figures The resulting formula, in the case of the elliptic integral of the third kind, is far from being simple, but it is practicable, and it requires none but the ordinary trigonometric and logarithmic tables This complexity is in reality due to the extremely complex character of the function itself, as is well known to every one conversant with its transformations My method becomes sufficiently simple when applied to complete elliptic functions of the first kind

My own opinion is that this method affords as easy an approximation as the nature of the elliptic and ultra elliptic integrals, at least in their general form, admits,—that it is simpler than the use of Jacobi's functions Θ or Υ, and that except in isolated cases, there is no advantage to be derived from the computation of tables of such auxiliary functions, so far as the mere computation of elliptic functions is concerned

II. "On the Lunar Diurnal Variation of Magnetic Declination at the Magnetic Equator" By John Allan Broun, F R S, Director of the Trevandrum Observatory Received March 28, 1860

This variation, first obtained by M Kreil, next by myself, and afterwards by General Sabine, presents several anomalies which require careful consideration, and especially a careful examination of the methods employed to obtain the results. The law obtained seems to vary from place to place even in the same hemisphere and in the same latitude, and this to such an extent, that, for example, when the moon is on the inferior meridian at Toronto it produces a minimum of westerly declination, while for the moon on the inferior meridian of Prague and Makerstoun in Scotland it produces a maximum of westerly declination. No two places have as yet given exactly the same result, though the result for each place has been confirmed by the discussion of different periods

In order to obtain the lunar diurnal action, it has been usual to consider the magnetic declination at any time as depending on the sun's and moon's hour-angles and on irregular causes Thus, if at conjunction, H_0 be the variation due to the sun on the meridian, and h_0 be that due to the moon on the meridian, H_1 the variation for the sun at 1^h, h_1 for the moon on the meridian of 1^h, and so on, it is supposed that we may represent the variations for a series of days by the following expressions, where the nearest values of h to the whole hour-angles are given —

1st day $H'_0 + h'_0 + x_0$ $H'_1 + h'_1 + x_1$ $H'_{23} + h_{22} + x'_{23}$
2nd day $H''_0 + h''_{23} + x''_0$ $H''_1 + h''_0 + x''_1$ $H''_{23} + h''_{21} + x''_{23}$

*n*th day. $H^n_0 + h^n_1 + x^n_0$ $H^n_1 + h^n_2 + x^n_1$ $H^n_{23} + h^n_{23} + x^n_{23}$,

where x is due to irregular causes, and n is the number of days in a lunation nearly

Summing these quantities we have approximately,

$$\Sigma H_0 + \Sigma^0_{23} h + \Sigma x_0, \quad \Sigma H_1 + \Sigma^0_{23} h + \Sigma x_1, \qquad \Sigma H_{23} + \Sigma^0_{23} h + \Sigma x_{23} \quad (A)$$

and the means are,

$$H_0 + \text{☾} + \frac{\Sigma x_0}{n}, \quad H_1 + \text{☾} + \frac{\Sigma x_1}{n}, \qquad H_{23} + \text{☾} + \frac{\Sigma x_{23}}{n} \quad . \quad (B)$$

Here the hourly means are affected by the constant due to the total action of the moon on all the meridians, and by variables depending on disturbing causes. If, on the other hand, we arrange the series as follows,

$$\begin{array}{lll} H'_0 + h'_0 + x'_0, & H'_1 + h'_1 + x'_1, & H''_0 + h''_{23} + x''_0 \\ H''_1 + h''_0 + x''_1, & H''_2 + h'_1 + x'_2, & H'''_1 + h'''_{23} + x'''_1 \\ & \vdots & \\ H^{n-1}_{23} + h^{n-1}_0 + x^{n-1}_{23}, & H^n_0 + h^n_1 + x^n_0, & H^n_{23} + h^n_{23} + x^n_{23}. \end{array}$$

Summing these quantities we have,

$$\Sigma^0_{23} H + \Sigma h_0 + \Sigma^0_{23} x^{(1)}, \; \Sigma^0_{23} H + \Sigma h + \Sigma^0_{23} x^{(11)} \; . \Sigma^0_{23} H + \Sigma h_{23} + \Sigma^0_{23} x^{(24)} \quad (C)$$

and for the means,

$$\Theta + h_0 + \frac{\Sigma x^{(1)}}{n-1}, \; \Theta + h_1 + \frac{\Sigma x^{(11)}}{n-1} \; \ldots \; . \Theta + h_{23} + \frac{\Sigma x^{(24)}}{n-1} \qquad (D.)$$

In this case Θ is the mean of $n-1$ observations, of which 24 give the true means for the total solar influence, and the remaining $n-25$ being equally distributed through the hour-angles also give the mean approximately

Instead, however, of combining the observations in this way, the following method has been preferred Let, in the quantities (B),

$$(H_0) = H_0 + ☾ + \frac{\Sigma x_0}{n}$$

$$(H_1) = H_1 + ☾ + \frac{\Sigma x_1}{n}$$

Then
$$\begin{aligned} H_0 + h'_0 + x_0 - (H_0) &= h'_0 + (x'_0) = d'_0 \\ H''_1 + h''_0 + x''_1 - (H_1) &= h''_0 + (x''_1) = d''_0 \end{aligned}$$

$$H_{23}^{n-1} + h_0^{n-1} + x_{23}^{n-1} - (H_{23}) = h_0^{n-1} + (x_{23}^{n-1}) = d_0^{n-1}$$

Summing the last two columns, we have

$$\frac{\Sigma d_0}{n-1} = h_1 + \frac{\Sigma_{23}^0 (x')}{n-1}$$

Similarly we obtain

$$\frac{\Sigma d_1}{n-1} = h_1 + \frac{\Sigma_{23}^0 (x')}{n-1}, \text{ and so on}$$

It will be observed that in these summations there are two assumptions, one, that the lunar diurnal law is constant throughout the lunation, or series of lunations, for which the means are obtained, or that the quantity ☾ in the expressions (B) is constant If this be not exact, then the quantity $\frac{\Sigma x}{n}$ will contain the variation due to this cause, and depend in part on the lunar hour-angle, so that the mean (H) which is employed in taking the differences will eliminate part of the lunar action and partially distort the law The other assumption is that the mean solar diurnal variation, represented by (H_0),(H_1) , is nearly constant throughout the period, for, if not, the differences due to such changes might be sufficient to mask any lunar law, the latter having a small range compared with the former

Also it should be remarked that the means h_0, h_1, &c are combined with the irregular effect $\frac{\Sigma_{23}^0 (x)}{n-1}$ This effect, as far as it is due to disturbance, we know obeys a solar diurnal law, and if independent of lunar action, a sufficiently large series of observations might suffice to eliminate it, as combining with and forming part of the regular solar diurnal variation. If, however, the series is not very large and the irregular disturbance considerable compared with the variation sought, it may be desirable to omit or modify the marked irregularities.

As regards the first assumption referred to above, the results obtained hitherto seem to show the error to be small, and the only way to determine its amount will be to consider it zero in the first instance, and thereafter a more accurate calculus may be employed. For the second assumption, it is certain that the solar diurnal law varies considerably in some cases within a lunation. At the magnetic equator, for example, the law of magnetic declination is inverted within a few weeks near the equinoxes. The attempt to correct the error due to considerable change in the solar diurnal variation by taking the means, as has been done, from shorter periods than a lunation, is liable to the serious objection that the resulting hourly means are affected unequally by the lunar action, so that the sums (A) take the form,

$$\Sigma H_0 + \Sigma_p^0 h + \Sigma x_0, \quad \Sigma H_1 + \Sigma_q^1 h + \Sigma x_1 \qquad \Sigma H_{23} + \Sigma_r^{23} h + \Sigma x_{23},$$

where the second term in each expression is a variable. In the discussion to which I am about to allude, the following plan has been followed. The hourly means for the following series of weeks were taken, namely—

m_1 from 1st, 2nd, 3rd, and 4th weeks of the year
m_2 „ 2nd, 3rd, 4th, and 5th „ „
m_3 „ 3rd, 4th, 5th, and 6th „ „

The means of m_1 and m_2 were then taken as normals for the 3rd or middle week, of m_2 and m_3 as normals for the 4th week, and so on: these means were then employed for the differences from the corresponding hourly observations of the weeks to which they belonged.

With reference to the irregular effect, it is evidently desirable that we should know in the first instance whether it may not be a function of the lunar, as well as of the solar, hour-angle, for this end it is essential in the first instance to obtain the result including all the supposed irregular actions, and afterwards to eliminate these in the best manner possible

In the discussion of the Makerstoun Observations I had substituted for certain observations, which gave differences from the mean beyond a fixed limit, values derived by interpolation from preceding and succeeding observations General Sabine in his discussions has rejected wholly the observations which exceeded the limit chosen by him The omission of observations accidentally or intentionally, and the taking of means without any attempt to supply the omitted observations by approximate values, require consideration

Let m be the true hourly mean for an hour h, derived from the complete series of n observations, let m' be the mean derived from $n-1$ observations, one observation o being accidentally lost, then

$$m'=\frac{nm-o}{n-1},$$

$$m=m'-\frac{m'-o}{n}=m'-\frac{m-o}{n-1}$$

If, however, we supply the omitted observation by an interpolation between the preceding and succeeding observations, and if the interpolated value be $o+v$, we have

$$m''=\frac{nm+v}{n},$$

$$m=m''-\frac{a}{n}$$

The comparative errors of m' and m'' are therefore

$$\frac{o-m}{n-1} \text{ and } \frac{v}{n}$$

We may for any given class of observation determine the mean values of these errors

Example:—At Hobarton, in July 1846, the mean barometer for 3^h (Hobarton mean time) was 29·848 in., and the mean difference of an observation at that hour from the mean for the hour was 0·403 in., if an observation had been omitted with such a difference, or for which $o-m=0·403$ in., we should have an error in the resulting mean of $\frac{0·403}{25}=0·016$ in., and the error might have been twice as great had the observation with the greatest difference been rejected. If we now seek the error of m'', where the observation is interpolated, we shall find for the same month that the mean value of $x=0·005$ in nearly, whence the error $\frac{x}{n}=\frac{0·005}{26}=0·0002$ in only, and the error would never exceed 0·001 in. A similar though less advantageous result will be found in all classes of hourly observations.

In the case where observations are rejected which differ from the mean for the corresponding hour more than a given quantity, let us suppose, to simplify the question, that the sums of $n-1$ out of n observations for each of two successive hours are each equal M, and that the observations for the same hours of the nth day are respectively $m'+l$ and $m'+l+x$, where $m'=\frac{M}{n-1}$, l is the limit beyond which observations are rejected, and x is the excess of the observation to be omitted. The means retaining all the observations are,

$$m_1=\frac{M+m'+l}{n}=m'+\frac{l}{n},$$

$$m_2=\frac{M+m'+l+x}{n}=m'+\frac{l+x}{n},$$

but if we reject the observation $m'+l+x$, we have

$$m_2'=\frac{M}{n-1}=m'$$

It is assumed that $m_1'-m_2'=0$ (any other hypothesis of variation would give the same final result), and therefore the error of the change from the first hour to the second, when all the observations are retained, is $\frac{n}{x}$, but if the observation be rejected, the change is

$$m'+\frac{l}{n}-m'=\frac{l}{n}$$

This error, therefore, will be greater than the other if $l>x$, so that the error in the resulting change from one hour to the next will be less by retaining an observation than by rejecting it, if the difference from the preceding observation be not greater than the difference from the hourly mean; that this will most frequently be the case will be obvious from the following fact:—At Makerstoun, in 1844, at 1 A.M. the number of observations which exceeded the monthly means by 3′ and less than double that, or 6′, was 99, while the whole number which exceeded by more than 6′ was only 16.

It will be evident also that the difference l of an observation from the corresponding hourly mean may not be due to irregular causes, or to causes which affect the changes from one hour to the next in a perceptible manner, but to gradual and regular daily change If we examine the *daily* means most free from irregular or intermittent disturbance, we shall find that they vary plus or minus of the monthly mean, if the difference amounts to l in any case, then the whole observations of the day may be rejected though they follow the normal law By taking a proper value of l this case may not happen frequently, but cases like the following will At Hobarton the daily means of magnetic declination differ in some months from the monthly means by 2'·0 nearly, as the limit chosen by General Sabine is 2' 4, any observation in such days differing by 0' 4 from the normal mean would be rejected The 25th and 26th days of March 1844 had been chosen by me as days free from magnetic disturbance, and following the normal law at Makerstoun (Mak Obs 1844, p 339), yet the means of horizontal force for these days differed 0 00064 and 0 00075 from the monthly means, had the former quantity been the limit, all the observations on these days might have been rejected

Altogether it appears to me that the method of rejecting observations beyond certain limits should not be employed at all, or if employed, only when interpolated observations are substituted, and that this interpolation should constitute a second part of the discussion, the first including all the observations*

These considerations may appear somewhat elementary, but it is essential that results which present such anomalies as the lunar diurnal variation of magnetic declination should be obtained in a manner the most free from objection, even though the objections should touch on quantities of a second order compared with those obtained

The discussion of which I now proceed to note the results, includes all the hourly observations without exception, made in the Trevandrum Observatory (within a degree and a half of the magnetic equator) during the five years 1854 to 1858, the second part of the discussion, in which days of great magnetic irregularity have been wholly rejected, not being completed, I shall reserve the details for a more formal communication to the Royal Society The results obtained are as follows —

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* I should note here my belief that a peculiarity noticed by General Sabine in his discussions as requiring explanation, namely, that the excursions of the declination needle east and west in the lunar diurnal variation have very different magnitudes, is due to the rejection of observations, while the means by which the differences were obtained included the rejected quantities

1st At the magnetic equator the lunar diurnal law of magnetic declination varies with the moon's declination and with the sun's declination.

2nd This variation is so considerable that the attempt to combine all the observations to form the mean law for the *year* gives results that are not true for any period Hence evidently the impossibility of relating the laws at different places The so-called *mean* law for the year at Trevandrum obtained for the moon furthest north, on the equator going south, furthest south, and on the equator going north, consists of *three* maxima and *three* minima,—a result wholly false, excepting as an arithmetical operation due to combination of very different laws

3rd The lunar diurnal law varies chiefly with the position of the *sun*, the variation being comparatively small with the position of the *moon*

4th At the magnetic equator the range of the variations is *markedly* greatest in the months of January, February, November and December, or about perihelion

The following results are derived after grouping the means for different positions of the moon in periods of six months, October to March, and April to September, they are therefore, for the reason given in the 2nd conclusion, not quite accurate, but the change of the law from month to month will be followed when the details are presented to the Society The following will give a general idea of the changes —

5th *When the moon is furthest north*

a About perihelion The lunar diurnal law of magnetic declination consists of two *maxima*[*] when the moon is near the upper and lower meridians, the maximum for the latter being much the greatest, of the two minima at intermediate epochs, that for the setting moon is the most marked

b About aphelion The law consists of two nearly equal *minima* near the upper and lower transits. of the two intermediate maxima, that near the moonset is the most marked

c. Thus the law about the winter solstice is inverted about the summer solstice, and the one law passes into the other at the epochs of the equinoxes, *exactly as for the solar diurnal variation*

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[*] The declination is easterly at Trevandrum, and the maxima indicate greater *easterly* declination

6th *For the moon on the equator going south*

a About perihelion The lunar diurnal law consists of two *nearly equal maxima* near the superior and inferior transits of the two intermediate minima, the moonset minimum is by far the most marked

b About aphelion The law consists of two *nearly equal minima* near the superior and inferior transits of the two intermediate maxima, that near moonrise is by far the most marked

c In this case also the laws for the solstices are the opposite of each other, and the one law passes into the other near the epochs of the equinoxes

7th *For the moon furthest south*

a About perihelion The lunar diurnal law consists of maxima near the upper and lower transits, that at the upper transit being by far the most marked of the intermediate minima, that near moonset is the greater

b About aphelion The law consists of two minima, the most marked at the inferior transit, the other about three hours before the superior transit, and of two equal maxima, one near moonrise, the other near the superior transit, but varying little till 3 hours before the inferior passage

c In this instance the inversion is not so complete as in the other cases, this, it is believed, will be found to be due to the fact that the change from one law to the other takes place after the vernal and before the autumnal equinox, so that in the means for six months, from which the above conclusions are drawn, the lunations following the law *a* are combined with those belonging to *b*

8th *The moon on the equator going north.*

a About perihelion The lunar diurnal law consists of two nearly equal maxima when the moon is near the superior and inferior meridians, of the two intermediate minima, that near moonrise is by far the most marked

b About aphelion. The law consists of two minima at the inferior and superior transits, and of two maxima, the greatest at moonset, the other between the meridians of 16^h and 21^h, between these points there is an inflexion constituting a slight minimum.

c. In this case also the opposition of the laws is sufficiently well marked; the only divergence from opposition being that due to the minor minimum about the meridian of 19^h, due, it is believed, as noted 7th *c*, to the partial combination of opposite laws in the aphelion half-year.

9th. It will be observed that the variations of the law with reference to the moon's declination *for any given period of the year*, consists chiefly in the difference of the relative values of the maxima and minima, the differences of epochs being small. Thus for perihelion, the moon furthest north, the principal maximum occurs at the inferior passage; the moon on the equator going south, the two maxima are nearly equal; the moon furthest south, the maximum at the superior passage is by far the greatest; on the equator going north, the two maxima are again nearly equal; and so on for other epochs.

10th. The moon's action is chiefly, if not wholly, dependent on the position of the sun, or (which is the same thing) on the position of the earth relatively to the sun; and the law of the lunar action at the magnetic equator resembles in some points that for the solar action at the same epochs. Thus about aphelion there is a *minimum* of easterly (maximum of westerly) declination produced by the lunar action, as well as by the solar action, for these two bodies near the superior meridian; whereas about perihelion both actions for the sun and moon near the superior meridian produce *maxima* of easterly declination. A like analogy holds for near the epochs of sunrise and moonrise.

III. Postscript to a Paper "On Compound Colours, and on the Relations of the Colours of the Spectrum." By J. Clerk Maxwell, Esq. Communicated by Professor Stokes, Sec. R.S. Received May 8, 1860.

(Abstract.)

Account of Experiments on the Spectrum as seen by the Colour-blind.

The instrument used in these observations was similar to that already described. By reflecting the light back through the prisms by means of a concave mirror, the instrument is rendered much shorter and more portable, while the definition of the spectrum is rather improved. The experiments were made by two colour-blind observers, one of whom, however, did not obtain sunlight at the time of observation The other obtained results, both with cloud-light and sun-light, in the way already described. It appears from these observations—

I. That any two colours of the spectrum, on opposite sides of the line "F," may be combined in such proportions as to form white

II That all the colours on the more refrangible side of F appear to the colour-blind "blue," and all those on the less refrangible side appear to them of another colour, which they generally speak of as "yellow," though the green at E appears to them as good a representative of that colour as any other part of the spectrum

III That the parts of the spectrum from A to E differ only in intensity, and not in colour, the light being too faint for good experiments between A and D, but not distinguishable in colour from E reduced to the same intensity The *maximum* is about $\frac{4}{9}$ from D towards E

IV Between E and F the colour appears to vary from the pure "yellow" of E to a "neutral tint" near F, which cannot be distinguished from white when looked at steadily

V At F the blue and the "yellow" element of colour are in equilibrium, and at this part of the spectrum the same blindness of the central spot of the eye is found in the colour-blind that has been already observed in the normal eye, so that the brightness of the spectrum appears decidedly less at F than on either side of that line, and when a large portion of the retina is illuminated with the light of this part of the spectrum, the *limbus luteus* appears as a dark spot, moving with the movements of the eye. The observer has not yet been able to distinguish Haidinger's "brushes" while observing polarized light of this colour, in which they are very conspicuous to the author.

VI Between F and a point $\frac{1}{9}$ from F towards G, the colour appears to vary from the neutral tint to pure blue, while the brightness increases, and reaches a maximum at $\frac{3}{8}$ from F towards G, and then diminishes towards the more refrangible end of the spectrum, the purity of the colour being apparently the same throughout.

VII. The theory of colour-blind vision being "*dichromic*," is confirmed by these experiments, the results of which agree with those obtained already by normal or "*trichromic*" eyes, if we suppose the "red" element of colour eliminated, and the "green" and "blue" elements left as they were, so that the "*red-making rays*," though dimly visible to the dichromic eye, excite the sensation not of red but of green, or as they call it, "yellow"

VIII The extreme red ray of the spectrum appears to be a sufficiently good representative of the defective element in the colour-blind When the ordinary eye receives this ray, it experiences the sensation of which the dichromic eye is incapable, and when the dichromic eye receives it, the luminous effect is probably of the same kind as that observed by Helmholtz in the ultra-violet part of the spectrum—a sensibility to light, without much appreciation of colour

A set of observations of coloured papers by the same dichromic observer was then compared with a set of observations of the same papers by the author, and it was found—

1 That the colour-blind observations were consistent among themselves, on the hypothesis of *two* elements of colour.

2 That the colour-blind observations were consistent with the author's observations, on the hypothesis that the two elements of colour in dichromic vision are identical with two of the three elements of colour in normal vision

3 That the element of colour, by which the two types of vision differ, is a red, whose relations to vermilion, ultramarine, and emerald-green are expressed by the equation

$$D=1\ 198V+0\ 078U-0{\cdot}276G,$$

where D is the defective element, and V, U and G the three colours named above.

IV. "Report to the Royal Society of the Expedition into the Kingdom of Naples to investigate the circumstances of the Earthquake of the 16th December 1857." By ROBERT MALLET, Esq., C.E., F.R.S.

(Abstract)

The region examined in this expedition, embraces, in its widest extent, most of the country between a line drawn from Terracina to Gargano on the north, down to the Gulf of Tarentum on the south.

The earthquake, the greatest that has occurred in Italy since that of 1783, was felt over nearly the whole of the Peninsula south of Terracina and Gargano Its area of greatest destruction (the meizoseismal area), within which nearly all the towns were wholly demolished, was an oval whose major axis was in a direction N W and S E nearly, and about 25 geog miles in length by 10 geog miles in width The first isoseismal area beyond this, within which buildings were everywhere more or less prostrated and people killed, is within an oval of about 60 geog miles by 35 geog miles, the second isoseismal is also an oval within which buildings were everywhere fissured, but few prostrated and few or no lives lost The third isoseismal embraces a greatly enlarged area, within which the earthquake was everywhere perceived by the unassisted senses, but did not produce injury A fourth isoseismal was partially traced, within which the shock was capable of being perceived by instrumental means, and which probably reached beyond Rome to the northward

The author divides his Report into three parts In the first he has developed the methods of investigation which he pursued for the purpose of finding the directions of movement of the wave of shock at various points, and thence to determine—1st The point upon the earth's surface vertically over the centre of effort or focal point, whence the earthquake impulse was delivered, 2nd, the depth below the surface (or rather sea-level) of the focal point itself The line passing through both these points he calls the seismic vertical The author points out, that of the three elements of the earthquake-wave, viz the velocity of transit, the velocity of the wave-particle (or wave itself), and the direction of motion at each point of the seismic area, the first alone in other instances has hitherto been attempted to be determined, the velocity of the wave and that of its transit being apparently confounded, and any attempt at direction confined to the apparent path on the surface. He then shows that every displaced object is in fact a seismometer, and that the displacement of regular bodies, such as buildings or their parts, may be made, by examination of their conditions after the shock, and the application of the principles of dynamics, to give precise information as to the true directions in azimuth and angles of emergence at various points of the wave, and its velocity at those points.

The effects produced, which are mainly available for such determinations, he shows are divisible into four great classes —1st. Fissures or fractures produced in buildings, from whose direction, &c that of the wave-path at the point may be discovered under this head the author has minutely described and figured the forms and peculiarities of fractures produced in all classes of buildings in the region examined. The principles deduced being universally applicable, he has shown the choice and precautions, &c as to those best fitted for seismic observation, and given formulæ for the deduction of the wave-paths, *i e* the direction in which the wave movement at the point emerges Velocity may also be determined from fissures, but this is more accurately ascertained from, 2nd, the overthrow of bodies, such as columns, piers, walls, &c, either fractured at their bases or simply overturned 3rd Fractures at the base without sensible movement, or with oscillation within an observed arc short of overthrow 4th The displacement of bodies by throw or projection, such as vases, finials, balustrades, bells, coping-stones, tiling, &c from elevated points, in which, where the vertical height fallen, and the horizontal range are observed, the velocity can be determined or the direction of the wave path and the angle of emergence of the wave, —*in certain cases all of these classes of displacement may occur* variously combined All these resolve themselves into fracturing forces, the movements of compound pendulums, and those of projectiles, and in arranging the formulæ for application, the author acknowledges the important assistance rendered him by his friend the Rev S Haughton, Professor of Geology in the University of Dublin

The author concludes the first part by a description of the characteristics of the towns and cities, buildings, &c in the region examined, of the physical features, the orographic and surface configuration, and the geological structure of the south of Italy as embraced in his investigation

The second part embraces the application of these methods of investigation, and the complete detail of the observations made by the author in his journey from point to point,—the working out at many separate points of the directions of the wave-paths and angles of emergence and wave-velocities,—the explications of the numerous and frequently singular and at first apparently perplexing circumstances, producing abrupt changes in the local intensity of seismic effects observed,—the description generally of the places examined and phenomena observed, embracing many examples of fissures in earth or rock, falls of rock, landslips, changes of water-courses, &c , and the explanation of their conditions,—the observations continually made to correct the magnetic declination for the observations of wave-path by compass,—the hypsometric determinations by the barometer of many points of elevation,—geological sections over certain parts of the country examined,—the time observations obtained for determination of transit velocity The descriptions are illustrated by numerous sketches made by the author on the spot, by diagrams and topical maps, and by a large series of photographs, made under the author's instructions, by a photographer who followed in his track

In the appendices to this part the author has given translations of all the notices that appeared in the 'Giornale Reale' (the only Neapolitan newspaper), of the events of the shock, with tables of the meteorology of Naples, from those of the Royal Marine Observatory, for certain periods before, during and after the shock , also returns of the population, area, damage, deaths and number of wounded persons in the shaken provinces

In the third and concluding part the author colligates all his facts, classifies them, and draws his conclusions and generalizations under the following heads ·—

a. The superficial position of the seismic vertical. This, the author shows from the independent and concurrent evidence of above 70 separate wave-paths, was close to the village of Caggiano, near the E extremity of the valley of the Salaris , the evidence being of a highly cumulative character, as the intersection of two wave-paths only is sufficient to determine this point.

b The depth of the focal point below the sea-level. This, the author shows, was about 5¾ geographical miles, *i. e* the *mean* focal depth.

c. The forms and areas of the meizoseismal and the several isoseismal curves. These have been laid down by the author upon three large maps, protracted from Zannoni's great map of the kingdom of the Two Sicilies, upon a scale of more than half an inch to the geographical mile.

Of these, the map A shows the wave-paths as determined in azimuth, and their close concurrence at the seismic vertical, the position of all the points of observation, and the axial lines of the great mountain ranges The map B gives the physical features of the country, and the four first isoseismal curves, distinguishing the injuries done to the numerous cities and towns, &c by separate colours Upon both maps the probable horizontal form of the focal cavity is marked, which coordinates with the existing lines of dislocation of the country in a remarkable manner The map C gives the whole of the isoseismals for this earthquake, and compares them with the corresponding seismal curves (so far as these can be obtained from the narratives), for a number of the greatest earthquakes on record which have occurred in the Italian Peninsula, including that of 1783

d The effects of the physical configuration of the surface and formations beneath, on the progress of the wave of shock, are discussed, and the peculiar oval forms and the directions of the major axes accounted for

e. The effects are pointed out, of the form and position of the focal cavity in modifying the distance of transmission of the wave of given effort in different directions

f Applies the results to showing the actual conformity of the isoseismal curves to the principles enunciated

g Under this head the author explains the nature of the separate system of wave-paths for Naples City, and the surrounding district, which only received the shock by transmission of refracted and transversal waves passed through the Monte St Angelo range of mountains, with entire change of direction

h. Colligates the facts ascertained as to the sounds heard with the shock at various points round the seismic vertical, and points out the remarkable relations that the prolongation of the sound more or less at different points bears to the direction in length and in depth of the focal cavity, and generally the causes of the diversity of sounds heard at various points around earthquake centres

i Discusses and points out the nature and correspondence with the dynamic laws of wave motions, of the tremors that preceded and followed the great shock, and generally the causes inducing such in all earthquakes.

k. Refers to the ascertained phenomena of reiterated or double shock at certain places, and points out the combinations, having reference to surface configuration chiefly, producing such from a single central impulse

Under the section *l*, the whole of the preceding information is combined, to deduce the dimensions, form and subterraneous position of the focal cavity, which the author shows to have been a curved lamellar cavity or fissure of about 3 geographical miles in depth by 9 geographical miles in length, with an inclined vertical section, and a mean focal depth (or depth of its central point of surface) of 5¾ geographical miles below the sea

In section *m* are discussed, upon the data of hypogeal increment of temperature (as supposed to be ascertained from deep mines and artesian wells), the necessary temperature of the focal cavity, and the intensity of the force that acted within it to produce impulse, assuming that to have been due to steam at high tension, either *suddenly* developed or *suddenly* admitted into a fissure rapidly enlarged by rending

n Deduces the amplitude of the wave and the work stored up in it on reaching the surface, and compares the former with the observed amplitudes

o Deduces the velocity of transit of the wave of shock upon the surface, from the most trustworthy of the observations of *time* at various localities These are found to correspond with considerable exactness, and give a transit rate of between 700 and 800 feet per second, as that at which the *wave form* was propagated from point to point, differing with change of formation by amounts stated

p. Deduces the velocity of the wave itself, *i e* that of the wave particle, which is shown to have been in round numbers between 13 and 14 feet per second (in the direction of the wave-path) A remarkable relation is pointed out between this velocity and that recorded for the earthquake of Riobamba, the greatest whose effects have been observed. The height due to the velocity of this wave is to the altitude of Vesuvius as that due to the velocity recorded of the Riobamba wave is to the mean height of the volcanic shafts of the Andes, and more especially to the height of the volcanic vents nearest to Riobamba The author points out that the *direct altitude* of a volcano is the true measure of the volcanic and seismic energy beneath it, and not its volume, which is a measure both of energy and time combined

Under section *q*, he discusses the facts ascertained by him, as to the decay of the wave of shock in relation to superficial distance from the seismic vertical The amplitude of the wave slowly and slightly increases, and its velocity decreases The observations are not sufficient to admit of certain deduction as to what function of the distance the law of decay follows The lowest velocity at nearly 30 miles from the seismic vertical was still about 11½ feet per second

Under *r*, the author has discussed systematically the facts ascertained as to the local disturbing causes producing abrupt perturbations of the wave of shock, and shown that they are —

1st Retardation by great fissures or faults, or deep valleys of dislocation, the effects of these, at about Muro and Bella, amounting almost to sudden extinction of the wave

2nd Alternate cutting off and partial extinction by parallel chains of mountains, and the effects of multiplied anti- and synclinals

3rd Increment and reduplication, with or without change of wave-path, by local reflection from mountain masses.

4th Effects of free-lying surfaces (flanks and extremities of mountain ranges) and nodal points, and production of intersecting secondary shocks, and sudden reductions of energy by entrance of the wave to greatly increased masses.

5th. Effects of formation (geological), of change from one to another, &c.

6th Effects of position of towns and cities on plain and hill, rock or loose material

Towns on steep rock eminences, such as Saponara, are shown to have suffered from an extreme conjoint velocity, viz that of the wave itself, and that of the hill-top, oscillating as an elastic pendulum.

Of all these modifying conditions, external contour of surface, and more especially the forms and directions, &c of mountain masses and deep fissures, are shown to be the most efficient. The value is shown of contoured maps, models, or other such means of directing the mind to the true figure of the country, in seismic researches.

In the section *s*, the author arranges and discusses the facts observed by him, as to secondary effects produced by the earthquakes, under the heads of—

1. Earth fissures and landslips. These are in directions generally more or less transverse to the wave-paths, but conform to and are

determined by the dip or slope of the subjacent beds of rock Earth fissures are *never* produced by the direct passage of the wave of shock. They are a mere secondary result, and are no more than incipient landslips

2. Rock shattering and falls.

3 Alterations of water-courses, muddying of springs, &c., all of which are shown to be due to the secondary effects of landslips into their beds, falls of partially loose rock therein producing ponding up and subsequent debacle

The total modifying effects on the earth's surface are shown to be insignificant

No great sea-wave accompanied this shock, nor was such possible, the focal point being inland The author examined with care more than 150 miles of sea-coast, as well as river-courses, for evidence of any permanent elevation of land having taken place concurrently with this earthquake, but found none Earthquakes cannot produce elevations, although the latter have been known to have taken place about the same time as earthquakes and in the same region

t Discusses the meteorological phenomena, both during the earthquake or directly after it, and for a prolonged period before it. Some remarkable relations are pointed out between the disturbance of the annual rainfall previously and the occurrence of shocks.

The physical conditions concerned in widely alleged unusual meteoric light, diffused over the central portion of the shaken region at the night of the shock, and of the occurrence of oppressive heat, &c., are discussed , and under *u*, the premonitory and other effects on lower animals, of nausea in men, &c , are considered

v. Points out that the method of investigation pursued, enables deductions even now to be drawn from ancient fissures, &c., as to the focal centres of earthquakes occurring at very remote periods In map D, the lines of loci of the focal points for the whole of the Italian peninsula are, as far as practicable, laid down, and their general connexion with the seismic bands of the Mediterranean Basin (as deduced from the British Association Earthquake Catalogue, and its accompanying Seismological Map of the World) is pointed out , and some general relations both of the unequal distribution of the points of greatest energy along these seismic bands, and of the unequal evolution of energy at the same points, in long periods of time, to the common origin of volcanic and seismic force, and its nature, are pointed out

Some observations of a practical engineering character are added, as to the proper construction of houses, &c in earthquake countries, by which the author is satisfied that the disastrous loss of life at intervals recurring might be avoided In conclusion, the author returns thanks to various individuals for co-operation in the objects of his expedition, and most especially to his friends Dr Robinson, Prof Haughton, General Sabine, Sir Roderick Murchison, and Sir Charles Lyell

June 7, 1860

The Annual Meeting for the Election of Fellows was held this day

Sir BENJAMIN C BRODIE, Bart., President, in the Chair

The Statutes relating to the Election of Fellows having been read, Sir Philip Egerton and Mr Babbage were, with the consent of the Society, nominated Scrutineers to assist the Secretaries in examining the lists

The votes of the Fellows present having been collected, the President announced that the following gentlemen were duly elected into the Society :—

Frederick Augustus Abel, Esq
Thomas Baring, Esq.
John Frederic Bateman, Esq.
Edward Brown-Séquard, M.D.
Richard Christopher Carrington, Esq
Francis Galton, Esq.
Joseph Henry Gilbert, Esq
Sir William Jardine, Bart
Thomas Hewitt Kev, Esq
Joseph Lister, Esq
Rev Robert Main, M.A.
Robert William Mylne, Esq
Roundell Palmer, Esq , Q.C
John Thomas Quekett, Esq
Edward Smith, M.D

The Society then adjourned to Thursday, June 14.

June 14, 1860.

General SABINE, R A., Treasurer and Vice-President,
in the Chair.

Francis Galton, Esq., Joseph Henry Gilbert, Esq., Thomas Hewitt Key, Esq., Joseph Lister, Esq., The Rev. Robert Main, Robert William Mylne, Esq., and Edward Smith, M.D , were admitted into the Society.

The following communications were read:—

I. "Notes of Researches on the Poly-Ammonias."—No. VIII. Action of Nitrous Acid upon Nitrophenylenediamine. By A. W. HOFMANN, LL.D., F.R S. Received April 5, 1860

The experiments of Gottlieb have shown that dinitrophenylamine, when boiled with sulphide of ammonium, is converted into a remarkable base, crystallizing in crimson needles, generally known as nitrazophenylamine, and for which, in accordance with the views I entertain regarding its constitution, I now propose the name Nitrophenylenediamine. I owe to the kindness of Dr. Vincent Hall a considerable quantity of this substance, which is not quite easily procured.

I have made a few experiments with this compound in the hope of obtaining some insight into its molecular constitution. If, bearing in mind the numerous analogies between the radicals ethyl and phenyl, we assume that the latter, by the loss of hydrogen, may be converted into a diatomic molecule, phenylene C_6H_4, corresponding to ethylene, the existence of a group of bases corresponding to the ethylene-bases cannot be doubted

$$\text{Ethylamine}\ \left.\begin{matrix}C_2H_5\\H\\H\end{matrix}\right\}N^{*}.\quad \text{Ethylenediamine}\ \left.\begin{matrix}(C_2H_4)''\\H_2\\H_2\end{matrix}\right\}N_2.$$

$$\text{Phenylamine}\ \left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N.\quad \text{Phenylenediamine}\ \left.\begin{matrix}(C_6H_4)''\\H_2\\H_2\end{matrix}\right\}N_2.$$

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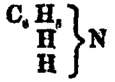
* H=1, O=16; C=12, &c

With the last-named body agrees in composition the compound known as semibenzidam, or azophenylamine, which Zinin obtained by exhausting the action of sulphide of ammonium on dinitrobenzol.

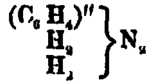
Those chemists, however, who have had an opportunity of becoming acquainted with the well-defined properties of ethylenediamine, will not be easily persuaded to consider the uncouth dinitrobenzol-product—sometimes appearing in brown flakes, sometimes as a yellow resin, rapidly turning green in contact with the air—as standing to smooth phenylamine in a relation similar to that which obtains between ethylenediamine and ethylamine, we much more readily admit a relation of this description between phenylamine and Gottlieb's crimson-coloured base, in which the clearly pronounced character of the former is still distinctly visible, although of necessity modified by the further substitution which has taken place in the radical

Phenylamine



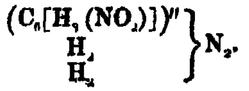
$$\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N$$

Phenylenediamine



$$\left.\begin{matrix}(C_6H_4)''\\H_2\\H_2\end{matrix}\right\}N_2$$

Nitrophenylenediamine



$$\left.\begin{matrix}(C_6[H_3(NO_2)])''\\H_2\\H_2\end{matrix}\right\}N_2.$$

Does the latter formula really represent the molecular constitution of the crimson needles? The degree of substitution of this body might have been determined by the frequently adopted process of ethylation. But even a simpler and a shorter method appeared to present itself in the beautiful mode of substituting nitrogen in the place of hydrogen, lately discovered by P Griess. The red crystals undergo, indeed, the transformation, which he has already proved for so many derivatives of ammonia, with the greatest facility.

On passing a current of nitrous acid into a moderately concentrated solution of the nitrate of the base, the liquid becomes slightly warm, and deposits on cooling a considerable quantity of brilliant white needles, the purification of which presents no difficulty: sparingly soluble in cold, readily soluble in boiling water, the new compound requires only to be once or twice recrystallized. Thus purified, this substance forms long prismatic crystals, frequently interlaced, white as long as they are in the solution, but assuming a slightly yellowish tint when dried, and especially when exposed to 100°. they are readily soluble both in alcohol and in ether. The new body exhibits a distinctly acid reaction, it dissolves on application of a gentle heat in potassa and in ammonia, without, however, neutralizing the alkaline character of these liquids, it also dissolves in the alkaline carbonates, but without expelling their carbonic acid The new acid fuses at 211° C, and sublimes at a somewhat higher temperature, with partial decomposition The sublimate consists of small prismatic crystals

Analysis proves this substance to contain

$$C_6 H_4 N_4 O_2,$$

a formula which is confirmed by the analysis of a silver-compound,

$$C_6 (H_3 Ag) N_4 O_2,$$

and of a potassium-salt,

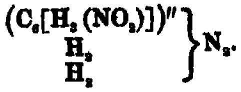
$$C_6 (H_3 K) N_4 O_2.$$

The analysis of the new compound shows that, under the influence of nitrous acid, nitrophenylenediamine exchanges three molecules of hydrogen for one molecule of nitrogen, three molecules of water being eliminated.

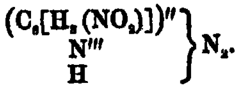
$$\underbrace{C_6 H_7 N_3 O_2}_{\text{Nitrophenylene-diamine.}} + H NO_2 = 2 H_2 O + \underbrace{C_6 H_4 N''' N_3 O_2}_{\text{New acid.}}$$

I do not propose a name for the new compound, which can claim but a passing interest, as throwing, by its formation, some light on the constitution of nitrophenylenediamine.

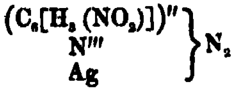
The composition of the new acid, and of its salts, shows that in the crimson-red base four hydrogen molecules are still capable of replacement, in other words, that this body contains four extra-radical molecules of hydrogen. The result of these experiments appears to confirm the view which, in the commencement of this Note, I have taken of the constitution of this body; at all events, the mutual relation of the several bodies is satisfactorily illustrated by the formulæ—

Nitrophenylenediamine 

$$\left.\begin{array}{c}(C_6[H_2(NO_2)])''\\ H_2\\ H_2\end{array}\right\}N_2.$$

New acid 

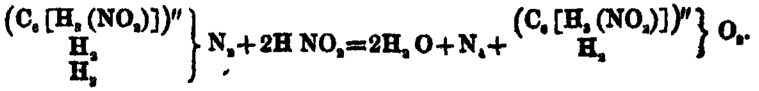
$$\left.\begin{array}{c}(C_6[H_2(NO_2)])''\\ N'''\\ H\end{array}\right\}N_2.$$

Silver-salt 

$$\left.\begin{array}{c}(C_6[H_2(NO_2)])''\\ N'''\\ Ag\end{array}\right\}N_2$$

If the admissibility of this interpretation be confirmed by further experiments, the reaction discovered by Griess furnishes a new and valuable method of recognizing the degree of substitution in the derivatives of ammonia

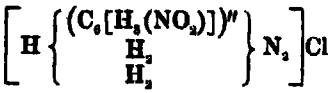
The new acid differs in many respects from the substances produced from other nitrogenous compounds As a class, these substances are remarkable for the facility with which they are changed under the influence of acids, and more especially of bases. The new acid exhibits remarkable stability, it may be boiled with either potassa or hydrochloric acid without undergoing the slightest change. Even a current of nitrous acid passed into the aqueous or alcoholic solution is without the least effect The latter experiment appeared of some interest, for if the action of nitrous acid, in a second phase of the process, had assumed the form so frequently observed by Piria and others, it might have led to the formation of the diatomic nitrophenylene-alcohol, according to the equation



$$\left.\begin{array}{c}(C_6[H_2(NO_2)])''\\ H_2\\ H_2\end{array}\right\}N_2+2HNO_2=2H_2O+N_4+\left.\begin{array}{c}(C_6[H_2(NO_2)])''\\ H_2\end{array}\right\}O_2.$$

It deserves to be noticed that nitrophenylenediamine, although derived from two molecules of ammonia, is nevertheless a decidedly monacid base. Gottlieb's analyses of the chloride, nitrate, and sulphate left scarcely a doubt on this point. However, as some of the natural bases, quinine for instance, are capable of combining with either one or two molecules of acid, I thought it of sufficient interest to confirm Gottlieb's observations by some additional experiments. The crystals deposited on cooling from a solution of nitrophenylenediamine in concentrated hydrochloric acid, were washed with the same liquid and dried *in vacuo* over lime.

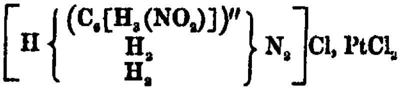
Analysis led to the formula



$$\left[H\left\{\begin{matrix}(C_6[H_3(NO_2)])''\\ H_2\\ H_2\end{matrix}\right\}N_2\right]Cl$$

The dilute solution of this chloride is not precipitated by dichloride of platinum, nor can the double salt of the two chlorides be obtained by evaporating the mixed solutions, which, just as Gottlieb observed it, is readily decomposed with separation of metallic platinum. I had, however, no difficulty in preparing a platinum salt, crystallizing in splendid long brown-red prisms, by adding the dichloride of platinum to the *concentrated* solution of the hydrochlorate

The platinum determination led to the formula



$$\left[H\left\{\begin{matrix}(C_6[H_3(NO_2)])''\\ H_2\\ H_2\end{matrix}\right\}N_2\right]Cl, PtCl_2$$

These experiments prove that, even under the most favourable circumstances, nitrophenylenediamine combines only with 1 equiv of acid, while the ethylene-derivatives are decidedly diacid. The diminution of saturating power in nitrophenylenediamine, at the first glance, seems somewhat anomalous, but the anomaly disappears if the constitution of the body be more accurately examined. It cannot be doubted that the diminution of the saturating power is due to the substitution which has taken place in the radical of the diamine. I have pointed out at an earlier period *, that the basic character of phenylamine is considerably modified by successive changes introduced into the phenyl-radical by substitution. Chlorphenylamine, though less basic than the normal compound, still forms well-defined salts with the acids, the salts of dichlorphenylamine, on the other hand, are so feeble, that, under the influence of boiling water, they are split into their constituents; in trichlorphenylamine, lastly, all basic characters have entirely disappeared. Again, on examining the nitro-substitutes of phenylamine, we find that even nitrophenylamine is an exceedingly weak base, whilst dinitrophenylamine is perfectly indifferent. What wonder, then, that a molecular system, to which in the normal condition we attribute a diacid character, should, by the insertion of special radicals, be reduced to monoacidity? The normal phenylenediamine, which remains to be discovered, will doubtless be found to be diacid, like the diamines derived from ethylene Even now the group of diacid diamines is represented in the naphtyl-series ·

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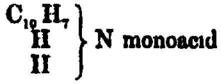
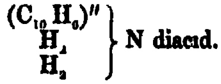
* Mem of Chem Soc vol ii. p. 298

Naphtylamine



$$\left.\begin{matrix} C_{10}H_7 \\ H \\ H \end{matrix}\right\} N \text{ monoacid}$$

Naphtylenediamine



$$\left.\begin{matrix} (C_{10}H_6)'' \\ H_2 \\ H_2 \end{matrix}\right\} N \text{ diacid.}$$

The body which I designate by the term Naphtylenediamine, is the base which Zinin obtained by the final action of sulphide of ammonium upon dinitronaphtaline This substance, originally designated seminaphtalidam, and subsequently described as naphtalidine, combines, according to Zinin's experiments, with 2 equivalents of hydrochloric acid*

II. "On the Formula investigated by Dr Brinkley for the general Term in the Development of Lagrange's Expression for the Summation of Series and for successive Integration." By Sir J F. W. Herschel, Bart., F.R.S. &c Received April 26, 1860.

(Abstract.)

In the Philosophical Transactions for the year 1807, Dr. Brinkley has investigated an expression of the general term of the series of Lagrange and Laplace for the finite differences and integrals of any function u in terms of its differential coefficients and common integrals of successive orders *ad infinitum*, which is in effect equivalent to the development of the functions $(e^t-1)^n$ and $(e^t-1)^{-n}$ in powers of t. The demonstration of the formulæ arrived at, as there stated, is circuitous and extremely difficult to follow, so much so as to render a simpler and easier one a desideratum in analysis, as there are probably few who have had the patience to follow it out to its conclusion.

More recently (Philosophical Transactions, 1816), the author of the present paper arrived at a general and extremely simple expression

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* Liebig's Annalen, vol. lxxxv. p. 328.

for the coefficient of t^x in the development of any function of e^t such as $f(e^t)$, in which are included, as particular cases, the two functions treated in Dr Brinkley's paper In the case of $(e^t-1)^n$, n being positive, the development so obtained agreed in form with that arrived at by Brinkley, and before him by Professor Ivory, but in that of $(e^t-1)^{-n}$ it differs from Brinkley's totally in point of form (though affording, of course, the same numerical results), being much simpler in expression and far more easily reduced to numbers. Neither was it at all apparent by what mode of transformation it was possible to pass from one form to the other, and this has ever since remained a difficulty

The essential difference between the two forms is, that in the general coefficient, as expressed by Brinkley, the progression of terms of which it consists are multiplied respectively by the successive differences of zero,

$$\Delta 0^{x+1}, \quad \Delta^2 0^{x+2}, \quad \Delta^3 0^{x+3}, \text{ \&c.,}$$

which run out in a diverging progression to infinity, so that the number of terms of which the coefficient consists is limited, not by this progression coming to an end *per se*, but by relations of another kind, whereas in the coefficient resulting from the other mode of treatment, the differences of zero involved form the progression

$$\Delta 0^x, \quad \Delta^2 0^x, \quad \ldots \Delta^x 0^x,$$

which terminates *per se* at its *x*th term. A theorem subsequently demonstrated by the author of this paper, however, in his "Collection of Examples in the Calculus of Finite Differences," affords the means of expressing any term in the former progression by a series of terms belonging to the latter. By substituting, then, the values so obtained for each of those which occur in Brinkley's series, the transformation in question is accomplished, and the process, which has the appearance of considerable complexity, is singularly simplified by the self-annihilation of all its most unmanageable terms.

III. "On the Thermal Effects of Fluids in Motion." By J. P. Joule, LL D, F.R.S., and Professor W. Thomson, LL.D., F.R.S. Received May 9, 1860.

In our paper published in the Philosophical Transactions for 1854, we explained the object of our experiments to ascertain the difference of temperature between the high- and low-pressure sides of a porous plug through which elastic fluids were forced. Our experiments were then limited to air and carbonic acid. With new apparatus, obtained by an allotment from the Government grant, we have been able to determine the thermal effect with various other elastic fluids. The following is a brief summary of our principal results at a low temperature (about 7° Cent)

Elastic fluid	Thermal effect per 100 lbs pressure on the square inch, in degrees Centigrade
Air.	1·6 Cold.
3 9 Air +96 1 Hydrogen	0 116 Heat.
7 9 Air +92 1 Nitrogen	1·772 Cold
5·1 Air +94 9 Oxygen	1 936 Cold.
3 5 Air +96 5 Carbonic acid	8·19 Cold
58 3 Air +41 7 Hydrogen	0 7 Cold.
62·5 Air +37·5 Carbonic acid	3·486 Cold.
54 6 Nitrogen +45·4 Oxygen	1·696 Cold.
4·23 Air +46 47 Hydrogen +49·3 Carbonic acid	2 848 Cold.

Further experiments are being made at high temperatures, which show, in the gases in which a cooling effect is found, a decrease of this effect, and an increase of the heating effect in hydrogen. The results at present arrived at indicate invariably that a mixture of gases gives a smaller cooling effect than that deduced from the average of the effects of the pure gases.

IV. "On the Nature of the Light emitted by heated Tourmaline." By BALFOUR STEWART, Esq., M.A. Communicated by J. P. GASSIOT, Esq. Received May 22, 1860.

Some months ago I had the honour of submitting to the Royal Society a paper on the light radiated by heated bodies, in which it was endeavoured to explain the facts recorded by an extension of the theory of exchanges.

Having mentioned the difficulty which I had in maintaining the various transparent substances at a nearly steady red heat for a sufficient length of time in experiments demanding a dark background, Professor Stokes suggested an apparatus by means of which this difficulty might be overcome, and it is owing to his kindness in doing so that I have been enabled to lay these results before the Society.

The apparatus consists of a thick, spherical, cast-iron bomb, about 5 inches in external and 3 inches in internal diameter—the thickness of the shell being therefore 1 inch. It has a cover removeable at pleasure. There is a small stand in the inside, upon which the substance under examination is placed, and when so placed it is precisely at the centre of the bomb. Two small round holes, opposite to one another, viz. at the two extremities of a diameter, are bored in the substance of the shell. If, therefore, the substance placed upon the stand be transparent, and have parallel surfaces, by placing these surfaces so as to front the holes, we are enabled to see through the substance, and consequently through the bomb. Let the bomb with the substance on the stand be heated to a good red heat, and then withdrawn from the fire and allowed to cool. It is evident that the cooling of the substance on the stand will proceed very slowly, as it is almost completely surrounded with a red-hot enclosure. It is also evident that, by placing the bomb in a dark room, we may view the transparent substance against a dark background. By this method of experimenting, therefore, the difficulty above alluded to is overcome.

Before describing the experiment performed on tourmaline, it may be well to state what result the theory of exchanges would lead us to expect when this mineral is heated, and we shall perceive at the same time the importance of the experiment with tourmaline as a test of the theory. When a suitable piece of tourmaline, with its faces cut parallel to the axis, is used to transmit ordinary light, the light which it transmits is nearly completely polarized, the plane of polarization depending on the position of the axis The reason of this is, that if we resolve the incident light into two portions, one of which consists of light polarized in a plane perpendicular to the axis of the crystal, and the other of light polarized in a plane parallel to the same axis, nearly all the latter is absorbed, while a notable proportion of the former is allowed to pass

Suppose now that such a piece of tourmaline is placed in a red-hot enclosure, the theory of exchanges, when fully carried out, demands that the light transmitted by the tourmaline, say in a direction perpendicular to its surface, *plus* the light radiated by the tourmaline in that direction, *plus* the small quantity of light reflected by the surface of the tourmaline in that direction, shall together equal in quantity and quality that which would have proceeded in the same direction from the wall of the enclosure alone, supposing the tourmaline to have been removed. Let us neglect the small quantity of light which is reflected from the surface of the tourmaline, and, standing in front of it, analyse with our polariscope the light which proceeds from it. This light consists of two portions, the transmitted and the radiated, both of which together ought to be equal in quality and intensity to that which would reach our polariscope from the enclosure alone were the tourmaline taken away. But the light which would fall on our polariscope from the enclosure alone would not be polarized, hence the whole body of light which falls upon it from the tourmaline, and which is similar in quality to the former, ought not to be polarized. Now part of this light, or that which is transmitted by the tourmaline, is polarized; hence it follows, in order that the whole be without polarization, that the light which is radiated should be partially polarized in a direction at right angles to that which is transmitted

Another way of stating this conclusion is this The light which the tourmaline radiates is equal to that which it absorbs, and this equality holds separately for light polarized in a plane parallel to the axis of the crystal, and for light polarized in a plane perpendicular to the same.

The experiment was made with a piece of brown tourmaline having a few opake streaks, procured from Mr. Darker of Lambeth. It was placed in a graphite frame between two circular holes made as above described in opposite sides of the bomb, the diameter of the holes being about $\frac{9}{10}$ths of an inch. On looking in at one of these holes you could thus see through the tourmaline and the opposite hole, or, in other words, see quite through the bomb. An arrangement was also made by which part of the tourmaline might be viewed with the graphite behind it.

The apparatus thus prepared was heated to a red or yellow heat in the fire, placed on a brick in a dark room, and the tourmaline viewed by a polariscope which Mr Gassiot kindly lent me The following was the appearance of the experiment —

Without the polariscope the transparent parts of the tourmaline were slightly less radiant than the field around them. When the polariscope was used, the light from the transparent portions of the tourmaline was found to vary in intensity as the instrument was turned round. No change of intensity could be observed in the light radiated by the opake streaks of the tourmaline, or by the graphite.

The light from the transparent portions was therefore partially polarized. The polariscope was then brought to its darkest position, and a light from behind allowed to pass through the tourmaline. The light was distinctly visible in this position, but by turning round the polariscope about 90° it became eclipsed. The mean of four sets of experiments made the difference between the position of darkness for the two cases 88½°. It appears, therefore, that the light radiated by the tourmaline was partially polarized in a plane at right angles to that which was transmitted by it. It was also ascertained that the light from the tourmaline which had the graphite behind it gave no trace of polarization.

V. "Researches on the Foraminifera."—Fourth and concluding Series. By W. B. Carpenter, M.D., F.R.S., F.G.S., F.L.S. &c. Received June 14, 1860.

(Abstract.)

The author in this communication brings to a conclusion that series of inquiries into the structural and physiological characters of *typical forms* of Foraminifera, which he had been induced to work out for the sake of turning to the account of Zoological science the valuable collections made by Mr. Jukes in the Australian Seas and by Mr. Cuming in the Philippine.

The first genus now treated of is *Polystomella*, the smaller and simpler forms of which have long been known, and of which the structure, so far as it can be elucidated by the examination of such specimens, has been already described with great care and accuracy by Professor W. C. Williamson. But in the comparatively gigantic and highly developed *Polystomella* of the Australian and Philippine series, a feature exists which is scarcely discernible in the humbler forms previously examined—that feature being the extraordinary development of the canal-system. A spiral canal runs along the inner margin of either surface of every whorl; from this canal a series of arches is given off, of which one passes down between every two adjacent segments, uniting it with the other spiral canal; whilst another set of straight branches passes directly towards the surface of the shell, through the thick calcareous deposit which covers in the depressed centre of the spire, and which extends as far as the last-formed spire. From the connecting arches, successive pairs of diverging branches proceed at frequent intervals; these, in the last whorl, make their way to the surface of the shell, and (when the shell is newly formed) open close on either side of the septal band; though, as the shell increases in thickness by subsequent deposit, the increased divergence of the branches separates their mouths from each other; and it very commonly happens that the two contiguous branches diverging from different arches meet and open by a single external pore half-way between the septal bands. When one whorl, however, has been surrounded by another, this radiating canal-system of the inner whorl does not usually continue itself directly into that of the outer (though such a continuation is not unfrequently seen), but the diverging canals for the most part terminate in the stolons of communication between the segments of sarcode that occupy the chambers of the outer whorl.

The evidence afforded by the distribution of the canal-system in *Polystomella* is decidedly confirmatory of the view expressed by the author on a former occasion, that this peculiar set of inosculating passages is related to the formation and nutrition of those solid calcareous layers which strengthen and connect the proper walls of the chambers, and to which he has given the designation of the "intermediate skeleton."

This view derives strong confirmation from the still more extensive distribution and greater importance of the canal-system of *Calcarina*, a genus of which Mr Cuming's Philippine collection affords a most remarkable series of illustrations This type may be considered as closely allied to *Polystomella* in the disposition and mode of communication of its chambers, save that the spire is generally more or less inequilateral Its "intermediate skeleton" is, however, much more developed, and it extends itself into a variable number of prolongations, sometimes simply club-shaped, sometimes more or less ramifying, which radiate in different directions from the central body, giving it somewhat the appearance of a spur-rowel, whence its generic designation. (An approach to this configuration is occasionally presented by the common *Polystomella crispa*, as also by some other species of *Polystomella*.) Now the independence of the intermediate skeleton and of the spiral system of chambers is curiously shown by the disproportionate development which they respectively exhibit the one to the other, and by their occasional complete disconnexion,—the spire altogether departing from its usual course, and (as it were) running wild, whilst the intermediate skeleton with its prolongations still present their ordinary configuration. The nutrition of the intermediate skeleton seems to be provided for by a system of large canals, freely inosculating with each other, which originate on the sides of the chambers, and are continued through the whole thickness of the intermediate skeleton, some of them passing directly to its nearest surface, whilst others are continued to the terminations of its radiating prolongations.

It is not a little remarkable that a Foraminiferous organism should present itself so extremely resembling the preceding as to be easily mistaken for it, and yet essentially differing from it in its plan of structure. This is the case with a type of which some remarkable specimens occur in Mr. Cuming's collection, and of which some smaller examples have been kindly put into the author's hands by Dr. J. E. Gray As it seems to be identical with the body described by Montfort under the designation *Tinoporus baculatus*, it may be right to retain that name, although it had been abandoned under the impression that it was a mere synonym of *Calcarina*. The structure of this body will be better understood after the description of a simpler form, which seems to be generally diffused through the seas of warmer latitudes, but of which the most remarkable examples present themselves in Mr. Jukes's Australian dredgings. Its shape is extremely variable, being sometimes an almost perfect sphere, in other cases resembling the lower half of a sugar-loaf, whilst in other cases again it is a very irregular depressed cone. It seems originally to have grown attached to zoophytes, corals, &c , since it frequently presents indications of such former attachment, though it is rarely to be met with otherwise than free It is, moreover, very closely allied in structure to the body which has been termed *Polytrema miniaceum*, under the belief that it was a Polyzoan Coral, but whose Foraminiferous affinities have been already perceived by Dr Gray, who has proposed for it the generic name of *Pustulipora*.

In the commencement of its growth, this organism seems closely to resemble *Planorbulina*, being formed of an assemblage of chambers arranged on one plane, spirally towards the centre, but irregularly clustered towards the circumference; each chamber communicating by single large septal orifices with the two contiguous chambers of the same row, whilst its walls are perforated with numerous large pseudopodian foramina This first-formed plane, however, is afterwards covered-in above and below by numerous successive layers of similar cells, which are piled one upon another in very regular rows; the original spiral type of growth being altogether lost in these superposed layers. In this mode the organism comes to present a near relationship to the fossil genus *Orbitoides**, the principal difference being that the superposed layers are not so completely differentiated from the original median layer in *Tinoporus* as they are in *Orbitoides*.

* See the author's account of the structure of that genus in the Quarterly Journal of the Geological Society, vol vi 1850, p 32

Now in *Tinoporus baculatus* we often find columns of solid shell-substance interposed between the angular partitions of the piles of superposed cells, just as they are in *Orbitoides*, their summits being visible on the surface as projecting tubercles, these columns are perforated with pseudopodian canals, which are extensions of the pores in the walls of the chambers over which they lie And the peculiar stellate projections which give to this species so much the aspect of a *Calcarina* are for the most part formed of a similar growth; for though the chambered structure is continued for a short distance as a conical protuberance into the base of each, yet this cone is invested and extended by a sheath of solid shell-substance, which is perforated by pseudopodian tubes extending through it from the chambers.

The last type of Foraminiferous structure described in this communication is one which appears to furnish a highly interesting link of connexion between *Foraminifera* and *Sponges* Its nature was at first entirely misunderstood, the specimens in Mr Cuming's collection having been supposed, not only by Mr Cuming, but by other conchologists, to be shells of a sessile Cirripede Their external resemblance might readily justify such an inference, since they are irregular cones, apparently composed of distinct valves, attached by a spreading base to the surface of shells or corals, and having a single orifice at their apex. A careful examination of the interior structure, however, makes it evident that the shell is multilocular, and that it is formed upon the type of the *Helicostègue* Foraminifera, closely resembling *Globigerina* in the commencement of its growth, the supposed 'valves' being the walls of the outer whorl, the chambers of which are very large, and are partially subdivided by incomplete septa All the principal chambers communicate by orifices of their own with a sort of central funnel which leads to the external orifice, and thus their relation to it is very much that of the separate orifices of the chambers of *Globigerina* to its umbilicus The cavities of the chambers are occupied by a spongeous tissue, which contains siliceous spicules, and although the possibility that this spongy substance may be parasitic must not be lost sight of, yet reasons are given which seem to render it almost certain that this is the proper body of the organism, on which Dr Gray, who first discerned its true affinities, has conferred the generic name of *Carpenteria*.

The author concludes with some general observations upon the mutual affinities of the "typical forms" of Foraminifera whose structure he has now elucidated, and he sums up the evidence which his examination of them has furnished in regard to the very wide range of variation which seems especially to characterise this group,—avowing his conviction that the only classification of it which can approach to a really natural arrangement, will be one founded upon the idea of "descent with modification" as the means by which an almost infinite variety of special forms has been evolved from a few fundamental types.

June 21, 1860

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Frederick Augustus Abel, Esq., Thomas Baring, Esq, John Frederic Bateman, Esq, Edward Brown-Séquard, M.D., Richard Christopher Carrington, Esq., and Roundell Palmer, Esq, were admitted into the Society

In accordance with notice given at the last Meeting, the Right Honourable George Augustus, Earl of Sheffield, was proposed for election and immediate ballot, and the ballot having been taken, his Lordship was declared duly elected.

The following communications were read:—

I "Experimental Researches on various questions concerning Sensibility" By E. Brown-Séquard, M D Communicated by Dr. Sharpey, Sec R.S. Received May 24, 1860

The first question I propose to examine relates to the duration of sensibility in parts of the body completely deprived of the circulation of blood.

This question has hitherto received but little attention from physiologists. It is true that many experiments have been made to ascertain how long sensibility remains in animals in which circulation is stopped by the application of a ligature round the large blood-vessels of the heart; but I do not know of any special research upon the duration of sensibility in a nerve in which there is a suspension of circulation. No doubt it has been occasionally observed, in experiments made with the view of ascertaining what effects are due to the ligature of the aorta, that sensibility persists in the nerves of the lower limbs much longer than irritability in the muscles, but no precise determination has been made of the exact duration of sensibility in such cases, except, to a certain extent, in some experiments of my own and those of Stannius My researches, although giving an indication of the duration of sensibility in the lower limbs, had not been made with the special view of elucidating this question, their object being to decide whether the vital properties of muscles and nerves could be restored after having completely disappeared The experiments of Stannius were made with the same view as mine It may therefore be said that the subject of the present paper is almost entirely new, at least as regards warm-blooded animals

A ligature round the aorta does not stop circulation completely enough to allow any positive conclusion regarding the duration of sensibility in nerves deprived of circulation of blood

Desirous of avoiding the causes of error which exist when the aorta is tied, I have proceeded in the following manner —I apply ligatures on the femoral artery, and after having divided this vessel between the ligatures, I amputate the thigh completely, excepting, however, that I leave the two large nerves of the limb undivided and as little injured as possible

In experimenting in this way, I find—

1st. That the duration of sensibility in the toes, in Rabbits, varies between twenty and twenty-three minutes

2nd That, in Guinea-pigs, the duration varies between forty and fifty minutes. I have seen sensibility lasting a little more than an hour in one case

3rd That, in Dogs, the duration of sensibility varies between thirty and thirty-five minutes.

It is a very remarkable fact that the duration of sensibility varies so much in animals so nearly related to each other as Rabbits and Guinea-pigs.

The second question I propose to examine relates to the influence of temperature on the duration of sensibility in parts deprived of the circulation of blood. It has been erroneously assumed that vital properties last longer in parts submitted to a temperature similar to that of the body, than in parts of which the temperature is very much lowered I have experimented on almost completely amputated limbs of Guinea-pigs, as in the preceding instances The limbs were placed in a vessel dipped into water at different temperatures The results have been as follows —

1st Water at 104° Fahr Duration of sensibility, in average forty-one minutes.

2nd Water at 80° Fahr Duration of sensibility forty-nine minutes.

3rd Water at 50° Fahr Duration of sensibility fifty-three minutes.

4th Water at 35° Fahr Duration of sensibility fifty-eight minutes

These results, which will not surprise persons who know the laws of the influence of heat and cold on the vital properties of the spinal cord, of motor nerves and muscles, clearly show that the lower the temperature, the longer sensibility persists in parts deprived of circulation.

The third question I have tried to solve, is whether an augmentation in the vital properties of the spinal cord is able to influence the duration of sensibility in a limb deprived of the circulation of blood It is known that when a transverse section is made upon the posterior surface of the spinal cord in a mammal, and especially in a rabbit, all the parts of the body which are behind the section become much more sensitive than they were previous to the operation I have made two series of experiments to find out if, in cases of this kind, the duration of sensibility in parts deprived of circulation would be increased

In one series of experiments I first divide the posterior columns of the spinal cord and then amputate all the parts of a hind limb except the nerves, while in another series I divide the spinal cord after having made the amputation. In both series I find that sensibility lasts notably longer than in animals in which the posterior columns have not been divided For instance, in rabbits, instead of twenty or twenty-two minutes, sensibility lasts thirty or thirty-five minutes, and in one case I have seen it still persisting, though very weak, after thirty-eight minutes, I did not in this instance ascertain how long it lasted

A very remarkable fact is that in a rabbit in which the spinal cord is in a normal condition, and in which the toes, after partial amputation as in the preceding experiments, are about losing the last appearance of sensibility, I find that there is a rapid and very notable return of this vital property if I divide the posterior columns of the spinal cord in the dorsal region. These experiments show that when sensibility seems to be lost in a part deprived of circulation, it is not completely so, but that the transmitted excitation which causes sensation is too slight to produce it, and that if in its way to the sensorium this excitation meets with a cause of increase, then sensation can be produced by it.

II. "On Quaternary Cubics." By the Rev. GEORGE SALMON. Communicated by ARTHUR CAYLEY, Esq. Received June 14, 1860.

(Abstract.)

In this paper quaternary cubics are discussed under the canonical form first given by Professor Sylvester,

$$ax^3+by^3+cz^3+du^3+ev^3,$$

where $$x+y+z+u+v=0.$$

The writer shows how, when quantics are thus expressed with a supernumerary variable, it is possible to form contravariants also expressed with a supernumerary variable, and such that for the variables, either in covariant or contravariant, we may substitute differentials with regard to the variables of the other. By the help of this principle, covariants, contravariants, and invariants of the cubic are formed with great facility. It is proved that a quaternary cubic has five fundamental invariants of the degrees respectively 8, 16, 24, 32, 40, as well as an invariant of the degree 100, whose square can be expressed in terms of the five fundamental invariants. The discriminant is also expressed in terms of the four first of these invariants. It is remarked that in the same manner as the theory of ternary cubics is analogous to the theory of binary quartics, so there are many analogies between the theory of quaternary cubics and that of binary quintics.

Four covariants are noticed of the first degree in the variables, by the aid of which expressions for the cubic can be obtained analogous

to what M Hermite has called the "four types" of binary quintics.

Other covariants of the cubic are discussed, and in particular a general expression is given for that covariant of the 9th order which geometrically represents a surface passing through the twenty-seven right lines on the surface of the third order represented by the cubic.

III. "On the Construction of a new Calorimeter for determining the Radiating Powers of Surfaces, and its application to the Surfaces of various Mineral Substances" By W. Hopkins, Esq, M A, F.R.S., &c Received June 1, 1860

(Abstract.)

When the author's Memoir on the Conductivity of various substances was presented to the Society, it was intimated to him on the part of the Council of the Society, that it might be advisable to determine absolute instead of relative conductivities, the latter only having been attempted in his previous experiments It is partly in consequence of this intimation, and partly from the desire to make his former investigations more complete, that the author has given his attention to the construction of a calorimeter which might serve for this purpose. His present memoir contains a description of this instrument, with the results obtained from its application to the surfaces of various substances.

The apparatus used by Messrs. Dulong and Petit was more delicate and complete than the simpler instrument devised by the author of this paper, but it was calculated only to determine the radiating powers of substances of which the bulb of a thermometer could be constructed, or with which it could be delicately coated. The only substances to which, in fact, it was applied, were glass and silver, the radiation taking place, in the first case, from the naked bulb of the thermometer, and, in the second, from the same bulb coated with silver paper. In these cases, too, it was the whole heat radiating in a given time from the instrument, and not that which radiated from a given area, that was determined. For this latter purpose the apparatus was not well calculated, on account of the difficulty of obtaining with accuracy the area of the surface from which radiation took place. The instrument here described can be easily applied to any plane radiating surface, while the area of that surface can be easily determined to any required degree of accuracy. The quantity of heat radiating under given conditions, from a unit of surface in a unit of time, can thus be easily ascertained. The paper contains a detailed description of the instrument, and of the experiments made with it.

The following are experimental results thus obtained,—the unit of heat being that quantity of heat which would raise 1000 grs. of distilled water 1° Centigrade. The formula is that of Dulong and Petit, where

$\theta =$ temperature of the surrounding medium (the air in these experiments), expressed in Centigrade degrees;

$t =$ the excess of the temperature of the radiating surface above that of the surrounding medium, in Centigrade degrees;

$p =$ pressure of the surrounding medium (the atmosphere in these experiments), expressed by the height of the barometer in metres;

$a = 1\cdot0077$, a numerical quantity which is always the same for all radiating surfaces and surrounding media.

Then if Q denote the quantity of heat, expressed numerically, which radiates from a unit of surface (a square foot) in a unit of time (one minute), we have the following results for the substances specified:—

Glass

$$Q = 9\cdot566\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{\cdot72}\right)^{45} t^{1\cdot233}$$

Dry Chalk

$$Q = 8\cdot613\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{72}\right)^{45} t^{1\cdot233}$$

Dry New Red Sandstone.

$$Q = 8\cdot377\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{72}\right)^{45} t^{1\cdot233}$$

Sandstone (building stone).

$$Q = 8\cdot882\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{\cdot72}\right)^{\cdot45} t^{1\cdot233}$$

Polished Limestone

$$Q = 9\cdot106\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{72}\right)^{45} t^{1\cdot233}$$

Unpolished Limestone (same block as the last)

$$Q = 12\cdot808\, a^{\theta}(a^{t}-1) + \cdot03720\left(\frac{p}{\cdot72}\right)^{45} t^{1\cdot233}$$

IV. "On Isoprene and Caoutchine" By C GREVILLE WILLIAMS, Esq. Communicated by Professor STOKES, Sec R S Received June 4, 1860

(Abstract)

This paper contains the results of the investigation of the two principal hydrocarbons produced by destructive distillation of caoutchouc and gutta percha

Isoprene

This substance is an exceedingly volatile hydrocarbon, boiling between 37° and 38° C., after repeated cohobations over sodium, it was distilled and analysed The numbers obtained as the mean of five analyses were as follows —

Experiment		Calculation		
Carbon	88 0	C^{10}	60	88 2
Hydrogen	12·1	H^8	8	11 8
			68	100 0

Three of the specimens were from caoutchouc and two from gutta percha The vapour-density was found to be at 58° C. 2 40 Theory requires, for $C^{10} H^8 = 4$ volumes, 2 35 The density of the liquid was 0 6823 at 20° C.

Action of Atmospheric Oxygen upon Isoprene

Isoprene, exposed to the air for some months, thickens and acquires powerful bleaching properties owing to the absorption of ozone On distilling the ozonized liquid, a violent reaction takes place between the ozone and the hydrocarbon All the unaltered hydrocarbon distils away, and the contents of the retort suddenly solidify to a pure, white, amorphous mass, yielding the annexed result on combustion .—

Experiment		Calculation		
Carbon . .	78·8	C^{10}	60	78 95
Hydrogen	10·7	H^8	8	10 52
Oxygen .	10 5	O	8	10 53
			76	100 00

This directly-formed oxide of a hydrocarbon is unique, as regards both its formula and mode of production.

Caoutchine

Himly's analysis was correct. The mean results of three analyses are compared in the following Table with those of M. Himly:—

	Mean	Himly	Calculation		
Carbon	88·1	88·44	C^{10}	120	88·2
Hydrogen	11·9	11·56	H^{16}	16	11·8
				136	100·0

Two of the determinations, the results of which are incorporated in the above mean, were made on a substance from gutta percha. The vapour density was:—

Experiment	Himly	Calculation = 4 vols
4·65	4·46	4·6986

We now for the first time see the relation between the two hydrocarbons. It is the same as between amylene and paramylene. The author discusses the boiling-point of these bodies, and shows that they form most decided exceptions to Kopp's empirical law.

Action of Bromine on Caoutchine and its isomer Turpentine

Caoutchine and turpentine act on bromine in precisely the same manner. One equivalent of the hydrocarbon decolorizes four equivalents of bromine. To determine this point quantitatively, eight experiments were made, four with turpentine and four with caoutchine. The quantity of bromine-water employed was 20 cub. cents. =0·2527 gramme bromine.

Mean of four turpentine experiments	Mean of four caoutchine experiments
0·1074 grm.	0·1091 grm.

Conversion of Turpentine and Caoutchine into Cymole

By the alternate action of bromine and sodium on caoutchine or turpentine, two equivalents of hydrogen are removed, the final result being cymole, having exactly the odour hitherto considered characteristic of the hydrocarbon obtained from oil of cumin, and quite distinct from that of camphogene. The liquid was identified by the annexed analyses. No. I. was from turpentine, II. and III. from caoutchine.

	Experiment				Calculation		
	I	II	III	Mean.			
Carbon .	89·2	89·5	89·5	89·4	C^{20}	120	89·6
Hydrogen .	10·5	10·4	10·4	10·4	H^{14}	14	10·4
						134	100·0

Agreeing perfectly with the formula $C^{20} H^{14}$*

Paracymole.

At the same time that cymole is formed, there is a production of an oil having the same composition, but boiling about 300° C. The author has provisionally named it paracymole.

Action of Sulphuric Acid on Caoutchine.

Sulphuric acid acts on caoutchine, converting it almost entirely into a viscid fluid like hévéène, at the same time a very small quantity of a conjugate acid is formed, having the formula

$$C^{20} H^{16} S^2 O^6,$$

the composition was determined from that of the lime salt, which on ignition, &c., gave a quantity of sulphate of lime equal to 8·3 per cent. of calcium; $C^{20} H^{15} Ca S^2 O^6$ requires 8·5.

The author considers the action of heat on caoutchouc to be merely the disruption of a polymeric body into substances having a simple relation to the parent hydrocarbon. He deduces this view from the similarity in composition between pure caoutchouc, isoprene, and caoutchine.

The following Table contains the principal physical properties of isoprene and caoutchine:—

Table of the Physical Properties of Isoprene and Caoutchine.

Name	Formula.	Boiling-point	Specific gravity	Vapour-density.	
				Expt	Calculated
Isoprene	$C^{10} H^{8}$	37°	0·6823 at 20°	2·44	2·349
Caoutchine	$C^{20} H^{16}$	171°	0·8420	4·65	4·699

In the calculations rendered necessary by the numerous vapour-density determinations contained in this paper, and more especially in those "On some of the products of the Destructive Distillation of Boghead Coal," the author has so repeatedly had to ascertain the value of the expression $\frac{1}{1+0.00367\,T}$, that he was induced to calculate it once for all for each degree of the Centigrade thermometer from 1° to 150°. As it is always easy so to manipulate as to prevent the value of T falling between the whole numbers, the Table proved a most valuable means of saving time; the author has therefore appended it to his paper in the hope of its proving equally useful to other working chemists.

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* (Note received July 27.) Both the cymole from turpentine and that from caoutchine were converted into insolinic acid by bichromate of potash and sulphuric acid. The quantitative determinations made on the silver salt of the acid were almost theoretically exact.

V. "On the Thermal Effects of Fluids in Motion—Temperature of Bodies moving in Air." By J. P. Joule, LL.D., F.R.S., and Professor W. Thomson, LL.D., F.R.S. Received June 21, 1860.

(Abstract.)

An abstract of a great part of the present paper has appeared in the 'Proceedings,' vol. viii. p. 556. To the experiments then adduced a large number have since been added, which have been made by whirling thermometers and thermo-electric junctions in the air. The result shows that at high velocities the thermal effect is proportional to the square of the velocity, the rise of temperature of the whirled body being evidently that due to the communication of the velocity to a constantly renewed film of air. With very small velocities of bodies of large surface, the thermal effect was very greatly increased by that kind of fluid friction the effect of which on the motion of pendulums has been investigated by Professor Stokes.

VI. "On the Distribution of Nerves to the Elementary Fibres of Striped Muscle." By Lionel S. Beale, M.B., F.R.S., Professor of Physiology and of General and Morbid Anatomy in King's College, London, and Physician to King's College Hospital. Received June 19, 1860.

(Abstract.)

After alluding to the general opinions entertained with respect to

the termination of nerve-fibres in voluntary muscle, and to Kühne's recent observations, the author proceeds to state that his researches have led him to the conclusion that every elementary fibre is abundantly supplied with nerves, which form a network and lie upon the surface of the sarcolemma They do not penetrate through this membrane. The nerves never terminate in points, neither can any elementary fibres, or any part of a muscle, be found to which nerves are not freely distributed

The nerves run for the most part with the smaller arteries, and come into very close relation with the capillary vessels The elementary fibres of the tongue and diaphragm of the white mouse are nearly covered with nerve-fibres and capillaries Generally, the muscular fibres of mammalia and birds receive a much larger supply than those of reptiles and fishes The muscular fibres of some insects appear to receive a most abundant supply

As the nerve-trunks approach their distribution the individual fibres divide and subdivide, as was demonstrated long ago by Wagner. The fibres resulting from the subdivision often pursue a very long and complicated course by running parallel with other fibres derived from different trunks, until, after being traced for some distance, it is not possible to follow them Fine trunks composed of from three to seven or eight fibres can often be seen traversing the muscle The fibres pursue different directions, some dip down between the elementary muscular fibres, some pass and form with others from a different source small compound trunks, while others may be traced onwards for some distance, the individual fibres which gradually separate from each other being distributed to different parts, in succession, of several elementary muscular fibres. When the finest nerve-fibres can be seen passing round the elementary muscular fibre, they clearly consist of very delicate flattened bands.

Of the oval bodies or nuclei —Connected with all nerves in every part of the body, sensitive, motor, vascular, and probably in all animals, are little oval bodies or nuclei, which are the organs by which the nerves are brought into the closest relation with other textures The nerves multiply at their distribution by the division of these little bodies, and upon them their nutrition and the manifestation of the nervous phenomena depend. A great number is associated with perfection of nervous actions, and *vice versâ* They are found very freely connected with the vascular nerves, are abundant on those nerves near the ganglia from which they proceed, and in the ganglia themselves These bodies, with the nuclei of capillary vessels and those of fat vesicles, and probably other structures with peculiar cells, which alone deserve the name, have been included under the term "areolar tissue corpuscles" (*Bindegewebe-körperchen*) As specimens are usually prepared, it is quite impossible to distinguish these structures from each other It is probable that the gelatinous fibres, or fibres of Remak, are after all real nerve-fibres, and not a peculiar modification of fibrous tissue, as is now generally believed

The nerves and vessels, and with them, of course, the oval bodies, may be stripped off from the elementary muscular fibre They are in close contact with the sarcolemma, and the author has been led to conclude from some appearances he has observed, that this structure is really composed of capillaries and nerve-fibres, with intervening tissue

Of the manner in which nerves terminate —The fibres connecting the oval bodies or nuclei form with them a network, the branches of which are of course continuous with the subdivisions of the nerve-fibres The arrangement of the network, and especially the number and proximity of the nuclei to each other, differ materially in different localities On sentient surfaces the meshes are very small and the nuclei close together, but from the complexity and great number of the fibres, from the fact that many fibres which appear to be single can be resolved into three or four individual fibres, and from the circumstance of the network being imbedded, in most cases, in the midst of fibrous tissue, it is very difficult to describe its exact relations and disposition However, from the connexions of this network with the nerve-fibres, it would seem to follow that an impression made upon a given portion of a sentient surface might be transmitted to the nervous centre by contiguous fibres, as well as by the one which would form, so to say, the shortest route, and it is possible that impulses to motion may be conveyed to muscular fibres by a more or less circuitous path, as well as by a direct one

Of the so-called tubular membrane.—This is a transparent structure in which the nerve-fibres are imbedded It cannot strictly be called a membrane, because in many cases several fibres are imbedded in it, and often it is much thicker than the fibres it contains.

By examination with high powers (700 diameters), many fibres which appear to be single when seen by lower powers can be resolved into three or more, all enclosed in the same transparent tissue As the nerve-fibres approach their distribution, this transparent structure becomes much spread out It is intimately connected with nerve-fibres and capillaries, and with them forms a delicate expansion over the muscular fibres and in other parts, delicate fibres also, in connexion with the nerves and capillaries, may be observed in it. In some cases this expansion seems to be incorporated with the sarcolemma, and it is probable that in certain instances it is really the structure which has received that name

Axis cylinder and white substance.—The author has been led to conclude that, in consequence of the free division of the axis cylinder and white substance near the point of distribution of the nerve, a single fibre in the trunk of a nerve may carry impressions to or from a much larger extent of surface than is generally supposed. The white substance which surrounds the axis cylinder gradually diminishes, until, in the finer ramifications, it is impossible to say that a fibre consists of an axis cylinder and white substance, for its general appearance and refractive power are the same in every part, except where the nuclei are situated. The author considers that the definite characters of the axis cylinder and white substance in the trunks of the nerve, may be due to the gradual growth and altered relations of the fibres which occur during the development of the entire organism. In the ultimate ramifications the whole fibre seems to consist of a very transparent and perhaps delicately granular substance, but no *tubular membrane, medullary sheath, or axis cylinder* can be demonstrated as distinct structures

Of the formation of new fibres—In connexion with the terminal ramifications, new fibres are being continually developed by the division of the nuclei, and old ones undergo removal. The remains of the latter may, however, be seen in the form of very delicate fibres, in connexion with active nerve-fibres. The author regards much of the so-called connective tissue between the elementary fibres of muscle and in some other situations, as of this nature,—as the remains of structures whose period of functional activity was past, and which have been removed, all but this small quantity of insoluble material

The method of preparing the specimens is then briefly described. Observations were conducted principally on white mice, which were injected with the author's prussian blue fluid immediately after death[*] The paper concludes with the following summary of the most important facts elucidated in the inquiry.—

1 That nerve-fibres in muscle and in many other tissues, if not in all, may be traced into, and are directly continuous with, a network formed of oval nuclei and intermediate fibres.

2 That the organs by which nerves are brought into relation with other textures, and the agents concerned in the development of nerves and the formation of new fibres, are the little oval bodies or nuclei which are present in considerable number in the terminal ramifications of all nerves A great number of these bodies is associated with exalted nervous action, while, when they are sparingly found, we may infer that the nervous phenomena are only imperfectly manifested

3 That every elementary fibre of striped muscle is abundantly supplied with nerves, and that the fibres of some muscles receive a much larger supply than others

4 That the nerves lie, with the capillaries, external to, but in close contact with, the sarcolemma. They often cross the muscular fibre at right angles, so that one nerve-fibre may influence a great number of elementary muscular fibres There is no evidence of their penetrating into the interior of the fibre

The paper is illustrated with drawings, most of them magnified 700 diameters

VII. "On the Effects produced by Freezing on the Physiological Properties of Muscles" By MICHAEL FOSTER, B A., M.D. Lond. Communicated by Dr. SHARPEY, Sec. R.S. Received June 4, 1860.

The influence of cold upon animal life has been studied chiefly (as for example in reference to the phenomena of hybernation) at such degrees of temperature only as are insufficient to freeze the tissues. In cases of actual freezing, attention seems for the most part to have been directed to the organism as a whole, with a view of determining the question whether an animal apparently frozen to death could be revived The older writers[*] often allude to the revival of frozen insects as a familiar fact. Rudolphi[†] states that frozen Filariæ are brought to life upon thawing Franklin found frozen fishes revive on thawing, yet John Hunter[‡] never succeeded in restoring the animals he had frozen

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[*] The Microscope in its Application to practical Medicine, p 63.

One element of uncertainty in such experiments (in those on vertebrate animals at least) is the difficulty of making sure that the heart itself is frozen, without interfering with the expected result. In the experiments of a later observer, Duméril[§], it seems clear that the hearts of the frogs he froze and recovered, were not frozen, though the intestines were

Spallanzani[‖] seems to have been the only one among the older writers who studied the freezing of muscles removed from the influence of the general blood-current, and that only in an indirect way He revived irritability in the muscles of frogs, toads, and salamanders, which, after immersion for several hours in snow, had become rigid, "presque gelées," and gave no contraction upon being stimulated But he states that muscles frozen by a more intense cold lost their irritability for ever His means of stimulation were chiefly mechanical. There seems fair reason to suspect that the muscles which recovered their irritability were but partially frozen, had but partially lost their irritability, and would have exhibited a decided contraction when treated by those modifications of the galvanic stimulus which the modern physiologist has at his command. Spallanzani[¶], in another work, states that frozen snails died

Among later writers, the only authority I can find is Schiff, who states[**], "A sufficiently intense degree of cold will render muscles rigid, and yet so that they can be revived It is not clearly made out though whether this is accompanied by any contraction" And again, p. 46, "Frogs' muscles bear freezing without irretrievable loss of irritability for a longer time than they will exposure to that degree of heat which resembles frost in depriving them of irritability "

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[*] Reaumur, Whytt, Blumenbach, Spallanzani

[†] Histor Entoz t ii p. 62

[‡] Works by Palmer, vol iv. p 131

[§] Annales des Sciences Naturelles, 1852, vol xvii p. 7

[‖] Opuscules, tr Senebier, vol i. (ch vi) p 113

[¶] Letters on Respiration

[**] Lehrb s. 44.

Having myself made some experiments on this subject, I venture to lay them before the Society

Two methods were adopted to freeze the muscles In most instances I securely enclosed them in little bags of thin gutta percha, and so buried them in a mixture of salt and snow, or pounded ice At other times I suspended them in pure olive oil, contained in a small vessel surrounded by the freezing mixture, having previously ascertained that immersion for two hours at least in the same oil, at an ordinary temperature, had no injurious effect upon muscular irritability

The results I came to were as follows —

1 Completely frozen muscles are not irritable to the strongest stimulus we possess

2 Muscles which have been frozen for a short time only (five or ten minutes at the longest) may regain their irritability on being thawed

3 Muscles which have been frozen for more than ten minutes never regain their irritability

The loss of irritability seems to be due more to the occurrence of freezing than to any mere fall of temperature For although the irritability does diminish with the fall of temperature, and markedly so when the freezing-point of water is neared, yet the great loss and final extinction takes place only when the tissue itself is frozen

I know of no method of treating a muscle so as to lower the freezing-point of the water contained in its tissue, without so injuring it as to render such a procedure useless for the present purpose

In order that the irritability of any muscle should wholly disappear, the muscle must be wholly frozen A muscle may be in great part frozen, and yet capable of producing a movement when stimulated, by the contraction of the unfrozen part

The passage into the frozen state is accompanied by no contraction Frogs' limbs freeze exactly in the same position which they were previously maintaining, and when the individual muscles were frozen singly, I was unable to satisfy myself of the occurrence of any contraction Nor could I assure myself of the advent of any physiological rigidity distinguishable from the physical frozen state. The reaction of frozen muscle, as indicated by litmus paper, is neutral or faintly alkaline, thus differing from recently dead muscles

It is not the mere rigidity of the frozen muscle which mechanically, so to speak, prevents contraction, for irritability does not at once and fully return upon thawing An interval of time may with care be detected in most cases, during which the muscle is already thawed, but yet not irritable; and irritability returns gradually.

There is no exact relation between the duration of the frozen state and the duration and amount of the revived irritability It is not the case that a muscle frozen three minutes regains twice as much irritability, or remains afterwards irritable twice as long as a muscle frozen for six minutes The frost does not progressively exhaust as it were a given store, leaving, according as the operation is shorter or longer, more or less residue to be manifested when the muscle is thawed.

The amount of revived irritability will depend in great measure on the treatment the muscles experience when first thawed The receipt then of a slight shock, which afterwards it will bear with impunity, if not with advantage, may be sufficient to throw it back altogether into death.

Nor is the present a case of mere stoppage of the wheels of life, which, when once set in motion again, go on as well as before Muscles once frozen, however kindly treated, eventually die sooner than those left untouched There has been in the act of freezing a partial exhaustion of their vital forces

I ascertained, by section, that muscles which afterwards regained their irritability were frozen throughout Hence the revival of irritability cannot be supposed to depend upon any part having been left unfrozen. Nor, in muscles partially frozen, did the unfrozen parts seem to have any influence over the life of the frozen parts

In muscles which have been well frozen, the fibres are more readily separable from each other, and divisible into fibrillæ, than in those which die an ordinary death. Under the microscope, the fibres are clouded and more opake than usual, the transverse striæ generally invisible, and in some cases the whole sarcous tissue seems to be converted into a confused amorphous mass, lying loose in a sarcolemma, which is more strongly defined than usual

But these histological appearances have but little to do with the physiological phenomena. A muscle may be frozen so as to lose all irritability, and yet preserve its natural appearance Two muscles may be frozen so that both shall scarcely have a fibre that is not more opake than natural and that has not lost its striæ,—both, in short, shall be affected anatomically, as far as we can at present tell, to the same degree, and yet one will live and the other is dead.

In muscles which never regained their irritability, the act of thawing was accompanied by the onset of a peculiar rigor, which differed from the "rigor mortis," and resembled the "rigor caloris" in being an active contraction, i e. in producing a movement The hind leg of a frog, when rigor mortis comes on, retains the position it previously had, whether of flexion or extension Powerful excitation of the spinal cord or ischial plexus produces extreme extension Plunging into boiling water brings the flexed leg to extreme extension If the leg be killed by frost in a flexed condition, it will when thawed assume gradually the position of extreme extension so muscles, frozen singly, shorten when thawed This contraction is never seen in muscles destined to regain their irritability I have seen it come on in a muscle which had been frozen for three hours it is a sure sign of death. This contraction continues after the production of the movement as a peculiar rigidity, which vanishes only when the softening from decomposition becomes apparent.

The effect of low temperature on the frog's heart is very peculiar. There is a great diminution in the rate of rhythm, and very marked increase in the duration of each systole, so that sometimes the heart is frozen in a tetanic beat, as it were I have never seen a frozen heart resume its beat when thawed, but I have often seen one part of the ventricle still beating while another part was frozen quite hard.

Similar results were obtained by freezing the muscles of leeches and snails. Frozen for a short time they recovered their irritability, for a longer time they died

In the latter animals, not only was mere irritability recovered, but I have seen snails, which I had every reason, by examining the state of snails of the same size frozen under exactly the same conditions, to believe had been frozen throughout, regain voluntary motion, and crawl about with extended horns as if nothing had happened. Their slowly altered blood does not seem to lose its virtues by having passed into a state of ice

In the frog, the return of irritability is favoured by connexion with the general circulation. A frog was secured with its hind legs in a freezing mixture, the brain and spinal cord having been removed In a few minutes the legs were frozen stiff, and had lost all irritability After being frozen half an hour they were thawed Irritability returned

Nerves, too, like muscles, lose their excitability when frozen, and, like them, may regain it on being thawed if they have not been frozen too long I have always found a greater difficulty in recovering nerves than muscles.

One very curious thing is this, that, as Eckard states*, when nerves are frozen, the muscles to which they are distributed are thrown into contractions, and yet when muscles themselves are frozen, there is not only no tetanic spasm, but not necessarily even the smallest quivering

VIII. "On the alleged Sugar-forming Function of the Liver" By Frederick W. Pavy, M D. Communicated by Dr Sharpey, Sec R.S. Received June 21, 1860.

(Abstract)

This communication is an abridgement of a paper bearing the same title presented by the author to the Royal Society in 1858, with some additional matter, since disclosed by his experimental investigations.

He first shows, by analyses, that although the blood collected from the right side of the heart after death, as was formerly done, affords an abundant indication of the presence of sugar, yet that when it is removed from the same part by catheterism during life, it is found to contain but a trace of the saccharine principle. Inferences, therefore, that have been drawn of the *ante-mortem* state from *post-mortem* examinations must be abandoned as erroneous.

The heart excised instantaneously after sudden killing, contains blood as free from sugar as it is during life.

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* Eckard, Zeitschr. f. Rat. Med. vol. x. (1851).

Very slight causes are sufficient to determine the presence of a considerable amount of sugar in the circulation during life By simply interfering with the breathing, a strongly diabetic state of the urine may be induced in an hour To obtain a fair specimen of blood in its natural condition from the right side of the heart during life, the animal must remain in a perfectly tranquil state during the performance of catheterism

From some recent experiments, it would appear that the blood in the right side of the heart is not appreciably more saccharine than the blood in the portal vein

As in the case of the blood, the hitherto adopted mode of examination of the liver has, in the author's opinion, led to a fallacious inference of its physiological state It contains a material which is exceedingly susceptible of undergoing transformation into sugar by a process of the character of fermentation Acids, alkalies, extreme cold, and a heat sufficient to coagulate and destroy the ferment, check this transformation And by these agencies it may be shown, that if the liver contain any sugar at the moment of death, it is only to the extent of the merest trace. The saccharine state of the liver, which has been hitherto looked upon as belonging to life, is the result of a *post-mortem* change which takes place with an astonishing rapidity

The particular part played by the liver in reference to the point under consideration, is to form a material which was originally called the glucogenic substance As it is not considered by the author that this material is really a sugar-forming substance under physiological conditions, he has styled it hepatine, as belonging to the liver This substance is doubtless intended for a special and important purpose in the economy, but what this purpose precisely is, must be left for the present as an open question As one of its properties, it is most susceptible of undergoing transformation into sugar. Although in contact with a ferment, yet it resists, under natural circumstances during life, this kind of transformation. Immediately, however, that life is destroyed, the ordinary laws of chemistry come into operation, and a change into sugar is effected. Abnormal states of the circulation, and probably of the blood, also lead to a similar production. Certain altered states of the nervous system likewise occasion an extensive formation of sugar in the system.

After division of the spinal cord just below the phrenics, the temperature falls, and the transformation of hepatine into sugar after death is so slow, that the process is easily recognized under its true light

In the livers of animals naturally of low temperature, such as the frog, the oyster, and the mussel, it is a matter of the greatest facility to show that the organ is free from sugar during life, or at the period of death.

The ingestion of starchy and saccharine substances leads to a great accumulation of hepatine in the liver The liver itself becomes also greatly increased in size.

The average weight of the liver in eleven dogs fed upon an animal diet was $\frac{1}{30}$th that of the animal An analysis of seven of the livers gave an average per-centage of hepatine amounting to 7·19

Five dogs upon a vegetable diet gave an average weight of liver equal to $\frac{1}{15}$th that of the animal An analysis of three of the livers gave an average per-centage of hepatine amounting to 17·23

Four dogs upon a diet of animal food with a large admixture of sugar, gave an average weight of liver equal to $\frac{1}{10\frac{1}{2}}$th of that of the animal An analysis of the four livers gave a per-centage of hepatine precipitate amounting to 14·5.

Experiments made upon the rabbit confirm these results obtained upon the dog, showing that saccharine and amylaceous materials received as food are converted by the liver into hepatine The author looks upon this as a strong fact in opposition to the glucogenic theory. He conceives it to be in the highest degree improbable that sugar should be transformed into hepatine by the liver for the purpose of coming back again into sugar in the same organ.

In the transformation of hepatine into sugar in the liver after death, the average of four analyses gives a loss of one part and a half of hepatine for the production of one part of sugar

Hepatine stands in direct opposition to sugar in regard to its property of diffusibility. Its power of diffusion the author has found to be so low, that it does not pass at an ordinary pressure through animal membranes. Sugar and hepatine being mixed together and placed on one side of a piece of bladder in an endosmotic apparatus, the sugar diffuses itself and leaves the hepatine behind. This physical property of hepatine harmonises with its retention in the hepatic cells under a natural state of the circulation

Ligature of the portal vein causes the liver to become strongly saccharine. The blood also in the system gives a strong reaction of sugar, and in one experiment the urine yielded just a traceable indication with the copper solution The fact that the blood thus becomes saccharine on interrupting the flow of blood through the portal vein, stands directly in opposition to the result that might have been expected under the glucogenic theory

In a communication presented to the Royal Society in 1859, it was shown that injury to certain parts of the sympathetic system produces most rapidly a strongly diabetic state It has since been found that the introduction of carbonate of soda largely into the circulation altogether prevents this effect

IX "A new Ozone box and Test-slips." By E J Lowe, Esq., F.R A.S, F.L S &c. Communicated by John Lee, LL D.

Received April 16, 1860.

The ordinary form of Ozone-box being very cumbersome, the present one has been contrived to supersede it* The box is simple in construction, small in size, and cylindrical in form, the chamber in which the *test-slips* are hung is perfectly dark, and at the same time there is a constant current of air circulating through it, no matter from what quarter of the compass the wind is blowing The air either passes in at the lower portion of the box and travels round a circular chamber twice, until it reaches the centre (where the test-slips are hung) and then out again at the upper portion of the box in the same circular manner, or in at the top and out again at the bottom of the box

Fig. 1 represents a section of the upper portion of the box, showing

Fig 1

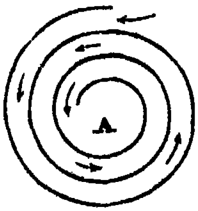
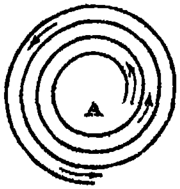


Fig 2



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* A specimen of the instrument was forwarded with the paper.

the manner in which the air enters and moves along to the centre chamber (where the test-slip is hung at A), and figure 2 represents a section of the lower half of the box where the air circulates in the opposite direction, leaving the box on the side opposite to that on which it had entered

The box has been tested and found to work well

On three different dates, when there was much ozone, test-slips were hung in one box, whilst others were hung in another which had the two entrances sealed up in order that no current of air should pass through, the result was satisfactory, viz .—

Example	New ozone-box	New ozone-box sealed up
1	10	0
2	9	0
3	9½	0

Then again, in five examples of test-slips being exposed without any box, in comparison with those placed in this new box, the result was —

Example	In new ozone-box	Exposed to north without a box
1	10	9
2	9	9
3	7	7
4	10	5
5	2	0

The ozone-box is capable of being suspended at an elevation above the ground, and this appears to be a great advantage, because elevation seems necessary in order to get a proper current of air to pass across the test-slips, indeed as an instance it may be mentioned, that at an elevation of 20 feet there is almost always more indication of ozone than at 5 feet.

The plan adopted here is to suspend the box to a T support, it being drawn up to its proper place by means of a thin rope passing over a pulley, and there is less trouble in examining and changing the test-slips in this manner than there was in the old method.

The box, as described, is made by Messrs. Negretti and Zambra of Hatton Garden.

It has been urged that a box was scarcely necessary for ozone test-slips, but as the papers fade on exposure to light, it must be evident that in order to register the maximum amount of ozone a *dark* box is required

Test-slips —Paper-slips being so fragile, I have substituted others made of calico The calico is to all intents and purposes chemically pure, containing only a few granules of starch, used in the first process of its manufacture, which it is very difficult to remove, being enveloped in the cotton fibre, it is, however, thought to be purer than the paper that is used for these test-slips, every precaution having been taken to make it so

Results of observations —The following Tables have been constructed from observations made between the 1st of May, 1859, and the 31st of March, 1860

TABLE I.

Mean amount of Ozone observed from Test-slips hung for twelve hours, both at night and in the daytime, in comparison with others hung for twenty-four hours

During the month of		Papers exposed for twelve hours			Papers exposed for twenty four hours.			Difference between twelve hours and twenty four hours	
		Day	Night	Difference	Day	Night	Difference		
1859	May	0·4	1·3	0·9	1·1	1·9	0·8	0·7	0·6
	June	0·8	0·9	0·1	1·3	1·5	0·2	0·5	0·6
	July	0·9	1·0	0·1	1·2	1·3	0·1	0·3	0·3
	August	0·7	1·4	0·7	1·2	1·8	0·6	0·5	0·4
	September	1·9	2·6	0·7	2·5	3·0	0·5	0·6	0·4
	October	0·5	0·7	0·2	0·7	0·9	0·2	0·2	0·2
	November.	1·5	1·7	0·2	1·8	2·1	0·3	0·3	0·4
	December.	1·7	2·0	0·3	2·1	2·5	0·4	0·4	0·5
1860	January	2·8	2·8	0·0	3·2	3·5	0·3	0·4	0·7
	February	2·3	2·8	0·5	2·6	3·0	0·4	0·3	0·2
	March	4·9	5·2	0·3	5·2	5·6	0·4	0·3	0·4
	Mean .	1·7	2·0	0·3	2·1	2·5	0·4	0·4	0·5

The ozone being always in excess in the night, and the tests exposed for twenty-four hours showing always an excess over those only exposed for twelve hours

TABLE II

Number of observations without any visible ozone

Month		During the night		During the day	
		Twelve hours' exposure	Twenty-four hours' exposure	Twelve hours' exposure	Twenty-four hours' exposure
1859	May	9	4	19	12
	June	18	10	15	9
	July	18	12	18	13
	August	10	4	15	9
	September	2	0	0	0
	October	16	12	18	14
	November	10	7	10	10
	December	10	5	7	5
1860	January	8	6	7	5
	February	12	6	9	9
	March	0	0	0	0
Number of days		113	66	118	86

Mean amount of ozone with the box suspended at the height of 25 feet

1859	December	24 hours' exposure =3 0	48 hours' exposure =5 0
1860	January	24 hours' exposure =3 9	48 hours' exposure =4 5
	February	24 hours' exposure =3·7	48 hours' exposure =5 4
	March	24 hours' exposure =5 9	48 hours' exposure =6 4

Mean amount of ozone with the box suspended at the height of 40 feet, March 1860, with twenty-four hours' exposure =7 1

X “On the Temperature of the Flowers and Leaves of Plants”

By E J LOWE, Esq , F.R.A.S , F L S &c. Communicated by THOMAS BELL, Esq , P.L S., V.P R.S. &c. Received April 16, 1860

(Abstract)

The present observations were made in order to ascertain whether different plants and flowers influence the temperature of the air immediately over them. The author was induced to undertake the inquiry from what he had noticed whilst making observations on the fall-cloud, or white mist of the valley, as it is usually called.

In the autumn of 1858 it was repeatedly noticed that vapour formed first over those fields from which hay had been gathered in the summer, and which were covered with a good crop of *after-math*.

The mists then gradually formed over the shorter grass of the pasture fields, yet, unless when the fog was very thick, it never formed over stubble fields (i e. where corn had grown) It was further observed, that at times when undrained or imperfectly-drained ploughed fields had this mist, those that were better drained were free from vapour, moreover, the furrows or low places of a field were the spots on which fogs first formed.

Hedges, on the contrary, seem to have a repulsive influence on fogs, the author has seen a field in which the mist rose higher than the hedges, but did not flow over or even touch them, but at length poured out through the gateway in a long dense column

These peculiarities seemed to be owing either to the different temperatures of different trees and vegetables, or of the soils on which they grew, or perhaps to both. To determine this, the author has tested as carefully as possible the temperature of various flowers in comparison with that of grass, as well as that of different soils, imitating artificially the conditions of drained and undrained fields, so that the observations might be made close together, and therefore better comparable with each other In this latter series, which is not yet completed, and does not form part of the present paper, daily records are made of the greatest heat and cold on the surface of soils, sand, gravel, clay, &c, as well as above the ground, and from 2 to 8 inches below the surface

The thermometers employed were constructed by Messrs Negretti and Zambra, they are self-registering, and entirely of glass, the degrees being engraved on the tube, and rendered more distinctly visible by means of enamel at the back Some of the instruments are of very small size, in order that they may be placed within the tubes of certain flowers

All the observations were carried on by myself at the Beeston Observatory

At first the thermometers were placed over the growing plant, and afterwards the flowers and leaves were arranged in bottles of water, and either exposed to full sunshine, or placed in the shade, the bottles being sunk in the ground to the level of the grass, filled with a bunch of flowers, and the thermometers placed immediately over them

With regard to the readings on the grass and on the common

Daisy (*Bellis perennis*), it should be mentioned that a portion of the grass was selected where a large group of daisies were fully in bloom, and within a foot of this another space where every daisy was carefully cut off from a circle of 12 inches, in the centre of which a second thermometer was placed on the grass, so that these records are very conclusive as regards the growing plant

Bearing upon this subject, it is proper to state, from a series of experiments with thermometers placed in full sunshine on the grass, and at 4 feet above the grass, that in winter the temperature on the grass is always lower than at 4 feet, whilst in summer the reverse takes place, the thermometers reading almost alike for a short time in spring and autumn To this circumstance may be attributed the less striking results in hot weather, especially where the difference between the grass and a flower is only from 1° to 2°

Tables stating the details of the observations, which extended from February 26 to September 19, 1859, and from the 22nd to the 27th of March, 1860, are given in the paper The author subjoins the following as the principal results :—

Great differences occur from time to time in the temperature of plants, and much depends upon the meteorology of the day, the differences being usually greater with a cloudless sky than with one which is loaded with cloud. The time of day seems also to operate on some plants to a great degree; as an instance, the *Erica herbacea*, which between 1 and 2 P M had shown a warmth of above 5° over that of the grass, by 3 o'clock was only 1° warmer, and by 4 o'clock was colder than the grass.

In the majority of instances grass is colder than flowers, as shown by the following examples —

1. Eleven observations on *Daphne cneorum* in comparison with grass.

The mean gives *Daphne cneorum* 1°·2 warmer than grass. The temperatures range between 41° and 73° 3. In nine cases the temperature was warmest over *Daphne cneorum*, the greatest difference being 3°·7.

2. Thirty-nine observations on *Gentiana acaulis* in comparison with grass

The mean gives *Gentiana acaulis* 2° warmer than grass. The temperatures range between 41° and 89°·8 In thirty-four cases the temperature was warmest over *Gentiana acaulis*, the greatest difference being 7° 9

3 Eight observations on *Iberis sempervirens* in comparison with grass

The mean gives *Iberis sempervirens* 2° 3 warmer than grass. The temperatures range between 41° and 63° 2. In seven cases the temperature was warmest over *Iberis sempervirens*, the greatest difference being 3° 1

4 Ten observations on *Alyssum tortuosum* in comparison with grass

The mean gives *Alyssum tortuosum* 2° 3 warmer than grass The temperatures range between 41° and 88° 9, in every case being warmer than grass, the greatest difference being 3° 7

5. Eleven observations of a white Daisy in comparison with grass

The mean gives the Daisy 0° 2 hotter than grass, the range of temperature from 40° 3 to 80° 5 Greatest difference 4° 4

6 Seven observations of *Veronica alpina* in comparison with grass.

The mean gives *Veronica alpina* 1° 9 warmer than grass, the temperature ranging from 42° 8 to 65°·5 In no instance was it lower than grass Greatest difference 5° 0.

7 Seven observations of *Alyssum tortuosum* in comparison with *Reseda frutescens*

The mean gives *Alyssum tortuosum* 2° 4 hotter than *Reseda frutescens*.

8 Thirty-three observations of *Tulipa Gesneriana* in comparison with grass.

The mean gives *Tulipa Gesneriana* 1° 9 hotter than grass, the temperature ranging between 31° 4 and 96° 5 In twenty-five instances it was warmer than grass Greatest difference 12°·8.

9. Twenty-eight observations of *Eschscholtzia crocea* in comparison with grass.

The mean gives the *Eschscholtzia* 3° 6 warmer than grass; the temperature ranging between 37° and 102° 3. In twenty-four instances it was warmer than the grass. Greatest difference with black bulb 15°·7.

10. Ten observations of *Lilium concolor* in comparison with grass.

The mean gives *Lilium concolor* 2°·9 warmer than grass; the temperature ranging between 64° and 85°·7. In every instance it was warmer, the greatest difference being 6° 6

11 Eleven observations of *Valeriana tuberosa* in comparison with grass

The mean gives the Valerian 0° 5 warmer than grass, the temperature ranging between 52° 6 and 73° In seven instances it was warmer than the grass, the greatest difference being 3° 3.

[In the last four examples the temperature at night, and the maximum with black bulb are included, so that the former will tend to lower the result and the latter to raise it]

XI "Reduction and Discussion of the Deviations of the Compass observed on board of all the Iron-built Ships and a selection of the Wood-built Steam-ships in Her Majesty's Navy, and the Iron Steam-ship 'Great Eastern';" being a Report to the Hydrographer of the Admiralty By FREDERICK J EVANS, Esq Communicated by the Lords Commissioners of the Admiralty Received May 5, 1860.

(Abstract.)

The analysis of the deviations of the compass in this paper comprises the observations made in forty-two iron ships, varying in size from 3400 to 165 tons, a selection of wood-built screw and paddle-wheel steam-vessels, as also the steam-ship 'Great Eastern' at various times prior to her departure from England.

The observations made in the iron-built ships extend over periods varying between thirteen and five years, and having been made with the same description of compass—the Admiralty standard—and under similar conditions of arrangement and situation, in accordance with the system carried out in Her Majesty's Navy, details of which are given, the general results are strictly comparable

In the analysis of the Tables, amounting to nearly 250 in number, of deviations observed in various parts of both hemispheres, the formula deduced from Poisson's General Equations by Mr. Archibald Smith, given in the Philosophical Transactions for 1846, p. 348, has been employed.

In this formula, the deviation of the compass on board ship,

reckoned positive when the north point of the needle deviates to the east, is given by the following expression —

$$\text{Deviation } (\delta) = A + B \sin \zeta' + C \cos \zeta' + D \sin 2\zeta' + E \cos 2\zeta',$$

ζ' being the azimuth (by compass) of the ship's head, reckoned from the magnetic north towards the east,

A, D, E being constant coefficients depending only on the amount, quality, and arrangement or position of the iron in the ship. B and C, coefficients depending on these, and also on the magnetic dip and horizontal intensity, are each consisting of two parts, one caused by the permanent magnetism of the hard iron, the deviation produced by which varies inversely as the horizontal force at the place, and the other, caused by the vertical part of the earth's force inducing the soft iron in the ship, the deviation produced by which varies as the tangent of the dip B representing that part of the combined attraction acting in a fore-and-aft direction, C that acting in a transverse, or athwart-ship direction

From the equation tan $\frac{{}^{-1}C}{B}$, the direction of the ship's force, and $\sqrt{B^2+C^2}$, the total magnetic force of the ship in proportion to the horizontal force at the place of observation is obtained for convenience, 1000 has been adopted to represent the value of the earth's horizontal force at the English ports of observation, in order, by an easy comparison, to note the changes on foreign stations

By comparison of the coefficients of the several descriptions of ships, it is observed that in wood-built steam-vessels, the coefficients B and C vary nearly as the tangent of the dip, from whence it may be inferred, as a general rule, that in steam machinery permanent magnetism bears but a small proportion to induced, but in iron-built ships, B and C generally vary more nearly as the inverse horizontal force, showing that they depend more on the permanent magnetism of the iron of the ship, and thus confirming the view of the Astronomer Royal, given in his earliest deductions (Phil. Trans. 1839), that the effect of transient induced magnetism is in these ships small comparatively. Numerous examples are given in detail of this permanency of magnetism, as also of the gradual diminution of the ship's force resulting from time.

An investigation of the coefficient D, which is caused entirely by the horizontal induction of the soft iron in the ship, and which is known as the "quadrantal" deviation, shows, that while in wood-built steam-ships it seldom exceeds 1° or 1½°, it rises in iron-built ships from 1½° to 6° and 7°, the Liverpool Compass-Committee recording even a point of the compass.

The chief characteristics of the quadrantal deviation, as developed in this investigation, are—

1 That it has invariably a positive sign, causing an easterly deviation in the N E and S W quadrants, and a westerly deviation in the S E. and N W. quadrants

2. Its amount does not appear to depend on the size, or mass of the vessel, or direction when building, or on the existence of iron beams

3. That a gradual decrease in amount has occurred, after the lapse of a number of years, in nearly every vessel that has been observed

4. That the value remains unchanged in sign and amount, on changes of geographic position

5. That a value not exceeding 4°, and ranging between that amount and 2°, may be assumed to represent the average or normal amount in vessels of all sizes.

Numerous examples are given in support of these propositions, as also of the uniformity of the amount of quadrantal deviation when determined in various parts of the ship, and, assuming the normal amount in iron steam-ships as from 2° to 4°, an analysis is given by which it is seen that *75* per cent. of the iron ships of the Royal Navy are included in this condition.

Two questions of importance here arise, are the results of this analysis conclusive, and if so, under what conditions do large quadrantal deviations occur? Reverting to the Astronomer Royal's early experiments in 1838–39, in the iron ships 'Rainbow' and 'Ironsides,' whose values were very small, and presuming that those vessels were built of good material—from their then experimental character—as also that similar conditions of material of good quality exist in the iron ships of the Royal Navy, it is assumed that the value (2° to 4°) represents the average condition of a ship built of the best or superior iron.

On the other hand, can the inference be drawn that large quadrantal deviation in an iron ship implies that inferior material has been used in her construction? Attention is here directed to the ships 'Birkenhead' and 'Royal Charter,' which from their well-known magnetic coefficients may be regarded as the types respectively of "hard" and "soft" iron constructed vessels, and from their consideration, as also from a review of the general results, these conclusions are derived:—

1 That in an iron ship of ordinary dimensions, a standard compass can be placed, the deviations of which will but little exceed those obtaining in wood-built steam-ships, and further, that on changes of geographic position, however distant, these deviations will be within smaller limits, and can be approximately predicted

2 A divergence from these conditions will arise when the inductive magnetism of the hull or machinery predominates, and it is inferred, especially from the example of the 'Royal Charter,' that large quadrantal deviation and fluctuating sub-permanent magnetism (due to hull alone) are co-existent, and give rise to conditions of compass disturbance which are beyond prediction, and which have hitherto baffled inquiry and given a complexion to theoretical deductions varying as regarded from different points of view

In order to examine the change which the original magnetism of an iron ship undergoes after launching, a series of compass observations were made in the steam-ship 'Great Eastern' prior to her quitting the River Thames in 1859, and subsequently at Portland, Holyhead, and Southampton*—at the three first-named places within short periods of time of each other

The results, from an Admiralty Standard Compass placed in a position the least subject to influence from local masses of iron, were as follows.—In the first five days, from Deptford to Portland, the ship's force had diminished from 0 585 to 0·480 [the earth's force =1·000], or nearly one-fifth, representing a decrease in the "semicircular" deviation from 35° 50′ to 28° 45′, the direction of the force, or neutral points, approaching the fore-and-aft line by 10°, or changing from 47° on the starboard bow to 37°.

At the expiration of the next six weeks, the ship in the interim having made the passage to Holyhead, the ship's force diminished from 0 480 to 0·390, or about one-sixth, corresponding to a decrease of "semicircular" deviation from 28° 45′ to 23° 0′, the direction of the force changing from 37° to 32°

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* The observations at Southampton were made after the paper was communicated to the Royal Society, and are introduced by way of supplement.

At Southampton, in June 1860, or nearly eight months after the experiments made at Holyhead, the force had further diminished from 0 390 to 0·235, or by one-half, corresponding to a decrease in the "semicircular" deviation from 23° 0′ to 13° 30′, whilst the direction of the force approached the fore and-aft line 25°, or from 32° to 7°, the quadrantal deviation remaining nearly constant [+4½°] the whole time included in the various observations

The unvarying tendency of the direction of the ship's force in the 'Great Eastern' to assume a fore-and-aft line, supports the view that time, with the vibrations and concussions due to sea service, leads to a distribution of the magnetic lines, of the nature of a stable equilibrium depending on the average of the inducing forces to which the ship is exposed, the respective sections of the hull having north and south polarity, being separated by lines approximating more nearly a horizontal plane and vertical axis through the body of the ship, instead of the inclined axis and equatorial plane of separation due to the magnetic dip of the locality, and divergence from the magnetic meridian, of the hull while building

The practical information resulting from the example of the 'Great Eastern' is, that prior to a newly built iron ship being sent to sea, her head during equipment should be secured in an opposite direction to that in which she was built, and that the magnetic lines should be assisted to be "shaken down" by the vibrations of the machinery in a short preparatory trip prior to the determination of her compass errors, or their compensation, but especially that in the early voyages vigilant supervision should be exercised in the determination of the compass disturbances

Another important point, generally neglected when compasses are adjusted by the aid of magnets in a newly built iron ship, is rendered manifest by the results of this investigation, namely, the necessity of the errors of the compass being determined and placed on record prior to the adjustment Without the knowledge to be derived from these observations of the magnetic force of the ship, all future changes of magnetism and consequent errors of the compass are mere guesswork both to those who adjust, and those in charge of the navigation of the ship.

It is recommended that, in any future legislation for the security of the navigation of our mercantile marine with reference to iron-built ships, the determination and record of these preliminary observations should be secured

The paper concludes by directing attention to the general principles of practical import which result from the investigation, viz as to the best direction with reference to the magnetic meridian for the keel and head of an iron ship to be placed in building, to ensure the least compass disturbance, the best position and arrangement for a compass to ensure small deviations, and permanency on changes of geographic position, and the changes to which the compass is liable from various causes on the foregoing conditions being fulfilled

For the best direction in building, it is shown that, from the nature of the polarity of the hull, and especially of the top sides in the after section of the ship and adjoining the compass, where usually placed, the latter is least affected in those vessels built in the line of the magnetic meridian

For iron steam-vessels engaged in the home or foreign trades in the northern hemisphere, it is recommended, from the then antagonistic magnetic influence of the hull and machinery, to build them head to the north for iron sailing vessels, from the top sides, in the usual position of the compass, being magnetically weak if built head to the south, the latter direction is to be preferred

The selection for the position of the compass depends on the direction of the ship during building, in those built head to north, it must be removed as far from the stern as convenience will permit, in those built head to south, as near to the stern as convenient, but avoiding especially, in all cases, proximity to vertical masses of iron In ships built head east or west, there is little choice of position in those built on the intercardinal points, a position approximating to the stern when the action from the top-sides—to be determined experimentally—is at a minimum, is to be preferred

Ample elevation above the deck and exact position in the middle line of the ship, are primary conditions to be observed; and no compass should be nearer iron deck beams than 4 feet. As every piece of iron not forming a part of, or hammered in the fabrication of the hull, such as the rudder, funnel, fastenings of deck houses, &c., is of a magnetic character differing from the hull of the ship, proximity to any such should be avoided, and, as far as possible, the compass should be so placed that they may act as correctors of the general magnetism of the hull

As most compasses are affected by the magnetism of the ship to an amount depending on their elevation, and the direction of the ship in building, the disturbances will be large comparatively, except in those vessels built head east or west

A series of Tables is appended, wherein the magnetic coefficients and ship's force and direction of the various classes of vessels are given, the ships being classed according to the nature of their material and machinery

XII. "On the Sources of the Nitrogen of Vegetation; with special reference to the Question whether Plants assimilate free or uncombined Nitrogen" By J B Lawes, Esq, F R.S , J H. Gilbert, Ph D., F R S , and Evan Pugh, Ph.D , F C.S, Received June 21, 1860

(Abstract)

After referring to the earlier history of the subject, and especially to the conclusion of Saussure, that plants derive their nitrogen from the nitrogenous compounds of the soil and the small amount of ammonia which he found to exist in the atmosphere, the Authors preface the discussion of their own experiments on the sources of the nitrogen of plants, by a consideration of the most prominent facts established by their own investigations concerning the amount of nitrogen yielded by different crops over a given area of land, and of the relation of these to certain measured, or known sources of it

On growing the same crop year after year on the same land, without any supply of nitrogen by manure, it was found that wheat, over a period of 14 years, had given rather more than 30 lbs —barley, over a period of 6 years, somewhat less—meadow-hay, over a period of 3 years, nearly 40 lbs —and beans, over 11 years, rather more than 50 lbs of nitrogen, per acre, per annum. Clover, another leguminous crop, grown in 3 out of 4 consecutive years, had given an average of 120 lbs. Turnips, over 8 consecutive years, had yielded about 45 lbs

The graminaceous crops had not, during the period referred to, shown signs of diminution of produce The yield of the leguminous crops had fallen considerably Turnips, again, appeared greatly to have exhausted the immediately available nitrogen in the soil The amount of nitrogen harvested in the leguminous and root crops was considerably increased by the use of "mineral manures," whilst that in the graminaceous crops was so in a very limited degree

Direct experiments further showed that pretty nearly the same amount of nitrogen was taken from a given area of land in wheat in 8 years, whether 8 crops were grown consecutively, 4 in alternation with fallow, or 4 in alternation with beans

Taking the results of 6 separate courses of rotation, Boussingault obtained an average of between one-third and one-half more nitrogen in the produce than had been supplied in manure His largest yields of nitrogen were in the leguminous crops, and the cereal crops were larger, when they next succeeded the removal of the highly nitrogenous leguminous crops In their own experiments upon an actual course of rotation, without manure, the Authors had obtained, over 8 years, an average annual yield of 57 7 lbs. of nitrogen per acre, about twice as much as was obtained in either wheat or barley, when they were, respectively, grown year after year on the same land The greatest yield of nitrogen had been in a clover crop, grown once during the 8 years, and the wheat crops grown after this clover in the first course of 4 years, and after beans in the second course, were about double those obtained when wheat succeeded wheat

Thus, cereal crops grown year after year on the same land, gave an average of about 30 lbs of nitrogen, per acre, per annum, and leguminous crops much more Nevertheless the cereal crop was nearly doubled when preceded by a leguminous one It was also about doubled when preceded by fallow Lastly, an entirely unmanured rotation had yielded nearly twice as much nitrogen as the continuously grown cereals

Leguminous crops were, however, little benefited, indeed frequently injured, by the use of the ordinary direct nitrogenous manures. Cereal crops, on the other hand, though their yield of nitrogen was comparatively small, were very much increased by direct nitrogenous manures, as well as when they succeeded a highly nitrogenous leguminous crop, or fallow. But when nitrogenous manures had been employed for the increased growth of the cereals, the nitrogen in the immediate increase of produce had amounted to little more than 40 per cent of that supplied, and that in the increase of the second year after the application, to little more than one-tenth of the remainder Estimated in the same way, there had been in the case of the meadow grasses scarcely any larger proportion of the supplied nitrogen recovered In the leguminous crops the proportion so recovered appeared to be even less, whilst in the root crops it was probably somewhat greater Several possible explanations of this real or apparent loss of the nitrogen supplied by manure are enumerated

The question arises—what are the sources of all the nitrogen of our crops beyond that which is directly supplied to the soil by artificial means? The following actual or possible sources may be enumerated:—the nitrogen in certain constituent minerals of the soil, the combined nitrogen annually coming down in the direct aqueous depositions from the atmosphere, the accumulation of combined nitrogen from the atmosphere by the soil in other ways, the formation of ammonia in the soil from free nitrogen and nascent hydrogen, the formation of nitric acid from free nitrogen, the direct absorption of combined nitrogen from the atmosphere by plants themselves, the assimilation of free nitrogen by plants

A consideration of these several sources of the nitrogen of the vegetation which covers the earth's surface showed that those of them which have as yet been quantitatively estimated are inadequate to account for the amount of nitrogen obtained in the annual produce of a given area of land beyond that which may be attributed to supplies by previous manuring Those, on the other hand, which have not yet been even approximately estimated as to quantity —if indeed fully established qualitatively—offer many practical difficulties in the way of such an investigation as would afford results applicable in any such estimates as are here supposed. It appeared important, therefore, to endeavour to settle the question whether or not that vast storehouse of nitrogen, the atmosphere, affords to growing plants any measurable amount of its *free* nitrogen. Moreover, this question had of late years been submitted to very extended and laborious experimental researches by M. Boussingault, M. Ville, and also to more limited investigation by MM Mène, Roy, Cloez, De Luca, Harting, Petzholdt and others, from the results of which diametrically opposite conclusions had been arrived at Before entering on the discussion of their own experimental evidence, the Authors give a review of these results and inferences, more especially those of M Boussingault who questions, and those of M. Georges Ville who affirms the assimilation of *free* nitrogen in the process of vegetation.

The general method of experiment instituted by Boussingault, which has been followed, with more or less modification, in most subsequent researches, was that adopted by the Authors in the present inquiry, namely, to set seeds or young plants, the amount of nitrogen in which was estimated by the analysis of carefully chosen similar specimens, to employ soils and water containing either no combined nitrogen, or only known quantities of it, to allow the access of free air (the plants being protected from rain and dust)—of a current of air freed by washing from all *combined* nitrogen—or of a limited quantity of air, too small to be of any avail so far as any compounds of nitrogen contained in it were concerned, and finally, to determine the amount of combined nitrogen in the plants produced, and in the soil, pot, &c., and so to provide the means of estimating the gain or loss of nitrogen during the course of the experiment

The plan adopted by the Authors in discussing their own experimental results, was—

To consider the conditions to be fulfilled in order to effect the solution of the main question, and to endeavour to eliminate all sources of error in the investigation

To examine a number of collateral questions bearing upon the points at issue, and to endeavour so far to solve them, as to reduce the general solution to that of a single question to be answered by the results of a final set of experiments.

To give the results of the final experiments, and to discuss their bearings upon the question which it is proposed to solve by them.

Accordingly, the following points are considered :—

1. The preparation of the soil, or matrix, for the reception of the plants and of the nutriment to be supplied to them

2. The preparation of the nutriment, embracing that of mineral constituents, of certain solutions, and of water

3 The conditions of atmosphere to be supplied to the plants, and the means of securing them, the apparatus to be employed, &c.

4. The changes undergone by nitrogenous organic matter during decomposition, affecting the quantity of combined nitrogen present, in circumstances more or less analogous to those in which the experimental plants are grown

5. The action of agents, as ozone, and the influence of other circumstances which may affect the quantity of combined nitrogen present in connexion with the plants, independently of the direct action of the growing process

In most of the experiments a rather clayey soil, ignited with free access of air, well-washed with distilled water, and re-ignited, was used as the matrix or soil In a few cases washed and ignited pumice-stone was used

The mineral constituents were supplied in the form of the ash of plants, of the description to be grown, if practicable, and if not, of some closely allied kind

The distilled water used for the final rinsing of all the important parts of the apparatus and for the supply of water to the plants, was prepared by boiling off one-third from ordinary water, collecting the second third as distillate, and redistilling this, previously acidulated with phosphoric acid

Most of the pots used were specially made, of porous ware, with a great many holes at the bottom and round the sides near to the bottom. These were placed in glazed stone-ware pans with inward-turned rims to lessen evaporation.

Before use, the red-hot matrix and the freshly ignited ash were mixed in the red-hot pot, and the whole allowed to cool over sulphuric acid The soil was then moistened with distilled water, and after the lapse of a day or so the seeds or plants were put in.

Very carefully picked bulks of seed were chosen, specimens of the average weight were taken for the experiment, and in similar specimens the nitrogen was determined

The atmosphere supplied to the plants was washed free from ammonia by passing through sulphuric acid, and then over pumice-stone saturated with sulphuric acid It then passed through a solution of carbonate of soda before entering the apparatus enclosing the plant, and it passed out again through sulphuric acid.

Carbonic acid, evolved from marble by measured quantities of hydrochloric acid, was passed daily into the apparatus, after passing, with the air, through the sulphuric acid and the carbonate of soda solution

The enclosing apparatus consisted of a large glass shade, resting in a groove filled with mercury, in a slate or glazed earthenware stand, upon which the pan, with the pot of soil, &c , was placed. Tubes passed under the shade, for the ingress and the egress of air, for the supply of water to the plants, and, in some cases, for the withdrawal of the water which condensed within the shade In other cases, the condensed water was removed by means of a special arrangement

One advantage of the apparatus adopted was, that the washed air was forced, instead of being aspirated, through the enclosing vessel. The pressure upon it was thus not only very small, and the danger from breakage, therefore, also small, but it was exerted upon the inside instead of the outside of the shade, hence, any leakage would be from the inside outwards, so that there was no danger of unwashed air gaining access to the plants.

The conditions of atmosphere were proved to be adapted for healthy growth, by growing plants under exactly the same circumstances, but in a garden soil The conditions of the artificial soil were shown to be suitable for the purpose, by the fact that plants grown in such soil, and in the artificial conditions of atmosphere, developed luxuriantly, if only manured with substances supplying combined nitrogen.

Passing to the subjects of collateral inquiry, the first question considered was, whether plants growing under the conditions stated would be likely to acquire nitrogen from the air through the medium of ozone, either within or around the plant, or in the soil, that body oxidating *free* nitrogen, and thus rendering it assimilable by the plants

Several series of experiments were made upon the gases contained in plants or evolved from them, under different circumstances of light, shade, supply of carbonic acid, &c. When sought for, ozone was in no case detected. The results of the inquiry in other respects, bearing upon the points at issue, may be briefly summed up as follows:—

1 Carbonic acid within growing vegetable cells and intercellular passages suffers decomposition very rapidly on the penetration of the sun's rays, oxygen being evolved

2. Living vegetable cells, in the dark, or not penetrated by the direct rays of the sun, consume oxygen very rapidly, carbonic acid being formed

3 Hence, the proportion of oxygen must vary greatly according to the position of the cell, and to the external conditions of light, and it will oscillate under the influence of the reducing force of carbon-matter (forming carbonic acid) on the one hand, and of that of the sun's rays (liberating oxygen) on the other Both actions may go on simultaneously according to the depth of the cell, and the once outer cells may gradually pass from the state in which the sunlight is the greater reducing agent to that in which the carbon-matter becomes the greater

4 The great reducing power operating in those parts of the plant where ozone is most likely, if at all, to be evolved, seems unfavourable to the oxidation of nitrogen, that is under circumstances in which carbon-matter is not oxidized, but on the contrary, carbonic acid reduced. And where beyond the influence of the direct rays of the sun, the cells seem to supply an abundance of more easily oxidized carbon-matter, available for oxidation, should free oxygen or ozone be present As nitrates are available as a source of nitrogen to plants, if it were admitted that nitrogen is oxidated within the plant, it must be supposed (as in the case of carbon) that there are conditions under which the oxygen compound of nitrogen may be reduced within the organism, and that there are others in which the reverse action, namely, the oxidation of nitrogen, can take place.

5 So great is the reducing power of certain carbon-compounds of vegetable substances, that when the growing process has ceased, and all the free oxygen in the cells has been consumed, water is for a time decomposed, carbonic acid formed, and hydrogen evolved.

The suggestion arises, whether ozone may not be formed under the influence of the powerful reducing action of the carbon-compounds of the cell on the oxygen eliminated from carbonic acid by sunlight, rather than under the direct action of the sunlight itself —in a manner analogous to that in which it is ordinarily obtained under the influence of the active reducing agency of phosphorus

But, even if it were so, it may be questioned whether the ozone would not be at once destroyed when in contact with the carbon-compounds present It is more probable, however, that the ozone said to be observed in the vicinity of vegetation, is due to the action of the oxygen of the air upon minute quantities of volatile carbo-hydrogens emitted by plants

Supposing ozone to be present, it might, however, be supposed to act in a more indirect manner as a source of combined and assimilable nitrogen in the Authors' experiments, namely,—by oxidating the nitrogen dissolved in the condensed water of the apparatus—by forming nitrates in contact with the moist, porous, and alkaline soil—or by oxidating the free nitrogen in the cells of the older roots, or that evolved in their decomposition

Experiments were accordingly made to ascertain the influence of ozone upon organic matter, and on certain porous and alkaline bodies, under various circumstances A current of ozonous air was passed over the substances for some time daily, for several months, including the whole of the warm weather of the summer, but in only one case out of eleven was any trace of nitric acid detected, namely, that of garden soil, and this was proved to contain nitrates before being submitted to the action of ozone.

It is not, indeed, hence inferred that nitric acid could under no circumstances be formed through the influence of ozone on certain nitrogenous compounds, on nascent nitrogen, on gaseous nitrogen in contact with porous and alkaline substances, or even in the atmosphere. But, considering the negative result with large quantities of ozonous air, acting upon organic matter, soil, &c., in a wide range of circumstances and for so long a period, it is believed that no error will be introduced into the main investigation by the cause referred to.

Numerous experiments were made to determine whether free nitrogen was evolved during the decomposition of nitrogenous organic compounds

In the first series of 6 experiments, wheat, barley, and bean-meal were respectively mixed with ignited pumice and ignited soil, and submitted for some months to decomposition in a current of air, in such manner that any ammonia evolved could be collected and estimated. The result was, that, in 5 out of the 6 cases, there was a greater or less evolution of free nitrogen—amounting, in two of the cases, to more than 12 per cent of the original nitrogen of the substance.

The second series consisted of 9 experiments, wheat, barley, and beans being again employed, and, as before, either ignited soil or pumice used as the matrix In some cases the seeds were submitted to experiment whole, and allowed to grow, and the vegetable matter produced permitted to die down and decompose In other cases, the ground seeds, or *meals*, were employed The conditions of moisture were also varied The experiments were continued through several months, when from 60 to 70 per cent of the carbon had disappeared

In 8 out of the 9 experiments, a loss of nitrogen, evolved in the pure state, was indicated In most cases, the loss amounted to about one-seventh or one-eighth, but in one instance to 40 per cent. of the original nitrogen. In all these experiments the decomposition of the organic substance was very complete, and the amount of carbon lost was comparatively uniform

It thus appeared that, under rare circumstances, there might be no loss of nitrogen in the decomposition of nitrogenous organic matter, but that, under a wide range of circumstances, the loss was very considerable—a point, it may be observed, of practical importance in the management of the manures of the farm and the stable

Numerous direct experiments showed, that when nitrogenous organic matter was submitted to decomposition in water, over mercury, in the absence of free oxygen, no free nitrogen was evolved. In fact, the evolution in question appeared to be the result of an oxidating process Direct experiments also showed, that seeds may be submitted to germination and growth, and that nearly the whole of the nitrogen may be found in the vegetable matter produced.

It is observed that in the cases referred to, in which so large an evolution of free nitrogen took place, the organic substances were submitted to decomposition for several months, during which time they lost two-thirds of their carbon. In the experiments on the question of assimilation, however, but a very small proportion of the total organic matter is submitted to decomposing actions apart from those associated with growth, and this for a comparatively short period of time, at the termination of which the organic form is retained, and therefore but very little carbon is lost. It would appear, then, that in experiments on assimilation no fear need be entertained of any serious error arising from the evolution of free nitrogen in the decomposition of the nitrogenous organic matter necessarily involved, so long as that is subjected merely to the exhaustion required to supply materials for growth in the ordinary process of germination On the other hand, the facts adduced afford a probable explanation of any small loss of nitrogen which may occur when seeds have not grown, or when leaves, or other dead matters, have suffered partial decomposition They also point out an objection to the application of nitrogenous organic manure in such experiments

Although there can be no doubt of the evolution of hydrogen during the decomposition of organic matter, and although it has long been admitted that nascent hydrogen may, under certain circumstances, combine with gaseous nitrogen and form ammonia, nevertheless, from considerations stated at length in the paper, the Authors infer that there need be little apprehension of error in the results of their experiments, arising from an unaccounted supply of ammonia, formed under the influence of nascent hydrogen given off in the decomposition of the organic matter involved

The Authors next consider the questions, whether assimilation of free nitrogen would be most likely to take place when the plant had no other supply of combined nitrogen than that contained in the seed sown, or when supplied with a limited amount of combined nitrogen, or with an excess of combined nitrogen? And again—whether at an early stage of growth, at the most active stage, or when the plant was approaching maturity? Combinations of these several circumstances might give a number of special conditions, in perhaps only one of which assimilation of free nitrogen might take place, in case it could in any

It is hardly to be supposed that free nitrogen would be assimilated when an excess of combined nitrogen is at the disposal of the plant It is obvious, however, that a wide range of conditions would be experimentally provided, if, in some instances, plants were supplied with no more combined nitrogen than that contained in the seed, in others brought to a given stage of growth by means of limited extraneous supplies of combined nitrogen, and in others supplied with combined nitrogen in a more liberal measure It has been sought to provide these conditions in the experiments under consideration.

In the selection of plants, it was thought advisable to take such as would be adapted to the artificial conditions of temperature, moisture, &c. involved in the experiment, also such as were of importance in an agricultural point of view—to have representatives, moreover, of the two great Natural Families, the Graminaceæ and the Leguminosæ, which seem to differ so widely in their relations to the combined nitrogen supplied within the soil—and finally, to have some of the same descriptions as those experimented upon by M. Boussingault, and M G. Ville, with such discordant results.

Thirteen experiments were made, 4 in 1857 and 9 in 1858, in which the plants were supplied with no other combined nitrogen than that contained in the original seed. In 12 of the cases prepared soil was the matrix, and in the remaining one prepared pumice.

Of 9 experiments with graminaceous plants, 1 with wheat and 2 with barley were made in 1857 In one of the experiments with barley there was a gain of 0 0016, and in the other of 0 0026 gramme of nitrogen In only two cases of the experiments with cereals in 1858, was there any gain of nitrogen indicated, and in both it amounted to only a small fraction of a milligramme Indeed, in no one of the cases, in either 1857 or 1858, was there more nitrogen in the *plants themselves*, than in the seed sown. A gain was indicated only when the nitrogen in the soil and pot—which together weighed about 1500 grammes—was brought into the calculation Moreover, the gain only exceeded 1 milligramme in the case of the experiments of 1857, when slate, instead of glazed earthenware stands were used as the lute vessels, and there was some reason to believe that the gain indicated was due to this circumstance In none of the other cases was the gain more than would be expected from error in analysis.

The result was then, that in no one case of these experiments was there any such gain of nitrogen as could lead to the supposition that *free* nitrogen had been assimilated. The plants had, however, vegetated for several months, had in most cases more than trebled the carbon of the seed, and had obviously been limited in their growth for want of a supply of available nitrogen in some form During this long period they were surrounded by an atmosphere containing free nitrogen, and their cells were penetrated by fluid saturated with that element It may be further mentioned, that many of the plants formed glumes and paleæ for seed

It is to be observed that the results of these experiments with cereals go to confirm those of M Boussingault

The leguminous plants experimented upon did not grow so healthily under the artificial conditions as did the cereals Still, in all three of the cases of these plants in which no combined nitrogen was provided beyond that contained in the original seed, the carbon in the vegetable matter produced was much greater than that in the seed—in one instance more than 3 times greater In no case, however, was there any indication of assimilation of free nitrogen, any more than there had been by the graminaceous plants grown under similar circumstances

One experiment was made with buckwheat, supplied with no other combined nitrogen than that contained in the seed. The result gave no indication of assimilation of free nitrogen.

In regard to the whole of the experiments in which the plants were supplied with no combined nitrogen beyond that contained in the seed, it may be observed that, from the constancy of the amount of combined nitrogen present in relation to that supplied, throughout the experiments, it may be inferred, as well that there was no evolution of free nitrogen by the growing plant, as that there was no assimilation of it, but it cannot hence be concluded that there would be no such evolution if an excess of combined nitrogen were supplied

The results of a number of experiments, in which the plants were supplied with more or less of combined nitrogen, in the form of ammonia-salts or of nitrates, are recorded Ten were with cereals, 4 in 1857, and 6 in 1858. Three were with leguminous plants, and there were also some with plants of other descriptions—all in 1858

In the case of the cereals more particularly, the growth was very greatly increased by the extraneous supply of combined nitrogen, in fact, the amount of vegetable matter produced was 8, 12, and even 30 times greater than in parallel cases without such supply. The amount of nitrogen appropriated was also, in all cases many times greater, and in one case more than 30 times as great, when a supply of combined nitrogen was provided The evidence is therefore sufficiently clear that all the conditions provided, apart from those which depended upon a supply of combined nitrogen, were adapted for vigorous growth, and that the limitation of growth where no combined nitrogen was supplied was due to the want of such supply

In 2 out of the 4 experiments with cereals in 1857, there was a slight gain of nitrogen beyond that which should occur from error in analysis, but in no one of the 6 in 1858, when glazed earthenware instead of slate stands were used, was there any such gain It is concluded, therefore, that there was no assimilation of free nitrogen In some cases the supply of combined nitrogen was not given until the plants showed signs of decline; when, on each addition, increased vigour was rapidly manifested In others the supply was given earlier and was more liberal

As in the case of the leguminous plants grown without extraneous supply of combined nitrogen, those grown with it progressed much less healthily than the gramınaceous plants. But the results under these conditions, so far as they go, did not indicate any assimilation of free nitrogen

The results of experiments with plants of other descriptions, in which an extraneous supply of combined nitrogen was provided, also failed to show an assimilation of free nitrogen

Thus, 19 experiments with cereals, 9 without and 10 with an extraneous supply of combined nitrogen—6 with leguminous plants, 3 without and 3 with an extraneous supply of combined nitrogen, and also some with other plants, have been made In none of the experiments, with plants so widely different as the graminaceous and the leguminous, and with a wide range of conditions of growth, was there evidence of an assimilation of free nitrogen.

The conclusions from the whole inquiry may be briefly summed up as follows:—

The yield of nitrogen in the vegetation over a given area, within a given time, especially in the case of leguminous crops, is not satisfactorily explained by reference to the hitherto quantitatively determined supplies of *combined* nitrogen.

The results and conclusions hitherto recorded by different experimenters on the question whether plants assimilate free or uncombined nitrogen, are very conflicting

The conditions provided in the experiments of the Authors on this question were found to be quite consistent with the healthy development of various Graminaceous Plants, but not so much so for that of the Leguminous Plants experimented upon.

It is not probable that, under the circumstances of the experiments on assimilation, there would be any supply to the plants of an unaccounted quantity of combined nitrogen, due to the influence either of ozone or of nascent hydrogen

It is not probable that there would be a loss of any of the combined nitrogen involved in an experiment on assimilation, due to the *evolution of free nitrogen in the decomposition of organic matter*, excepting in certain cases when it might be presupposed

It is not probable that there would be any loss due to the evolution of free nitrogen from the nitrogenous constituents of the plants during growth

In numerous experiments with graminaceous plants, under a wide range of conditions of growth, in no case was there any evidence of an assimilation of free nitrogen

In experiments with leguminous plants the growth was less satisfactory, and the range of conditions was, therefore, more limited. But the results with these plants, so far as they go, do not indicate any assimilation of free nitrogen It is desirable that the evidence of further experiments with such plants, under conditions of more healthy growth, should be obtained

Results obtained with some other plants, are in the same sense as those with graminaceous and leguminous ones, in regard to the question *of the assimilation of free* nitrogen

Seeing the evidence afforded of the non-assimilation of *free* nitrogen by plants, it is very desirable that the several actual or possible sources whence they may derive *combined* nitrogen should be more fully investigated, both qualitatively and quantitatively.

If it be established that plants do not assimilate free or uncombined nitrogen, the source of the large amount of combined nitrogen known to exist on the surface of the globe, and in the atmosphere, still awaits a satisfactory explanation.

XIII. "Observations made with the Polariscope during the 'Fox' Arctic Expedition." By David Walker, M.D., Surgeon to the Expedition,—in a Report transmitted by Sir Leopold McClintock. Communicated by Professor Stokes, Sec. R.S. Received March 7, 1860.

The observations made with the polariscope[*], with one exception, were confined to solar halos and parhelia. Several times I tried the instrument when lunar halos appeared, but the light was so faint that only once was I able to make an observation. The direction of the polarization was the same in all the cases observed, namely, in a plane parallel to a line joining the part looked at with the centre of the sun or moon. In several instances the instrument was turned round so as to find the plane of maximum polarization, but it was always found greatest in the parallel or perpendicular plane: when the plane was oblique, no perceptible polarization at all was perceived. The light was never completely polarized, the greatest amount not being more than half; such occurred on 21st of April, and 5th and 6th of May 1859. All the halos observed had a diameter of about 45°: there were some seen of diameter 90°, but the polariscope was not at hand at the time. Almost always the halos round the moon or sun were more or less prismatic, red internal. The observation on October 10th, 1857, was not on a halo, but the cloud which surrounded the moon at a distance of about $1\frac{1}{2}$° had circular and prismatic edges; light from these edges was slightly polarized, but not of the same image as in all the other instances.

Moon.

October 7th, 1857.—Polariscope applied to a halo round the moon, diameter about 45°; slight polarization; arrow and brighter image both being to the left when the instrument was held parallel to a line joining a part of halo to the left of the moon with the moon.

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[*] The polariscope employed consisted merely of a double-image prism of quartz, formed of two quartz prisms cut in the usual manner and cemented together, which was fixed at the eye-end of a tube about 18 inches long, provided at its other end with a rectangular aperture having its edges parallel and perpendicular to the planes of polarization of the two images, and of such breadth that the two images just touched each other along one edge. The plane of polarization of that image which was polarized in a plane passing through the axis of the instrument, was marked by an arrow at the eye-end, lying in that plane, and placed on the same side of the axis as the image in question.

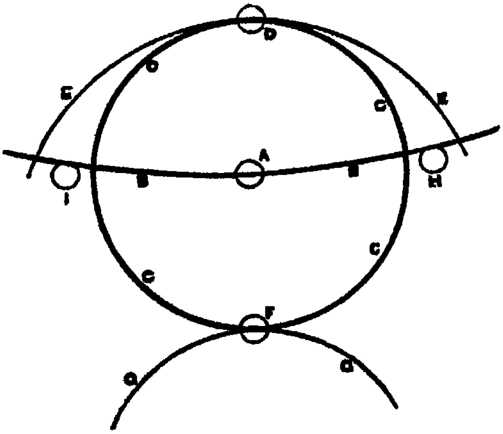
October 10th, 1857.—Polariscope applied to a prismatic luminous haze round the moon, three diameters of moon's radius, slight polarization, the image to the right hand being brighter, mark on eye-end to left hand, instrument held horizontal, looking at the haze to the right of the moon

Sun

March 18th, 1858 —Prismatic halo round the sun's diameter about 45°, with two parhelia, one on each side of the sun, instrument applied to the left parhelion, slight polarization, the outer or left image being brighter when the instrument was held in a plane parallel to a line joining the sun and parhelion, the eye-mark and brighter image both to the left

May 15th, 1858 —Prismatic halo round sun, diameter about 45°, with two parhelia, one on each side The instrument held in same azimuth as last observation, looking at left parhelion, a little stronger polarization, the outer image being brighter

May 25th, 1858 —Appearances in neighbourhood of sun and par-



A. Sun's altitude, 39° 15′.
B Circle running round heavens, 101° diam
C. Circle 22° in radius, passing round A and intersecting B
D A parhelion occurring on circle C.
E. An oval arc, radius horizontally 27° 30′, perpendicularly 22°
F. Parhelion occurring on circle C.
G. Part of a circle passing through F, 17° 15′ above horizon.
H and I. Two parhelia on horizontal circle (101° diameter), each distant from sun 25° 30′.

helia at 1·45 P M. —Sun's altitude 39° 15′. A circle of diameter 101° running round heavens and passing through the sun, intersected by another circle, radius 22°, having the sun for its centre Another intersection took place at 27° 30′ from the sun, on each side, by an arc (perpendicular), which coincided with the previous circle at an altitude of 22° from the sun Four parhelia appeared, one on each side of the sun, one above and one below, through the lower one passed part of an arc of another circle Polariscope applied to left-side parhelion and also to halo, slight polarization of outer or left image, instrument held in a plane parallel to a line joining parhelion and sun

July 16th, 1858, 10 40 P M —Prismatic parhelia 22° 30′ on each side of sun, altitude 7° Polariscope held in a parallel plane, parhelion on left of sun looked at, slight polarization, bright image and eye-mark both to the left hand

November 8th, 1858, 10 A M —Prismatic parhelion distant about 23° from each side of sun, altitude 6° Polariscope applied to left image, held horizontally (parallel), eye-mark to left, slight polarization, image to left and further from sun brighter Instrument held in same plane, but with eye-mark to right, applied to right parhelion, similar slight polarization, image to right and furthest from the sun brighter.

March 30th, 1859, 4 P M.—Prismatic parhelia about 23° distant from each side of sun. Polariscope applied to image on left of the sun, held parallel, eye-mark to left, slight polarization, image to left and further from the sun brighter Instrument held in same plane, but eye-mark to right; slight polarization, image to right and outer being the brighter′

April 21st, 1859, 7 P M —Prismatic parhelion and part of halo on each side of sun distant about 22° 30′ Polariscope applied to left-hand image, held in a parallel plane, eye-mark to left, *medium* polarization, the left or outer image brighter. Instrument held in same plane with eye-mark to right, and applied to right-hand image; similar *medium* polarization, image to right and further from the sun brighter.

May 1st, 1859, 6 P.M.—Prismatic parhelion with part of a halo on each side of sun distant 23° (about). Polariscope applied to left image, held parallel, eye-mark to left; slight polarization, left or outer image brighter. Instrument held in same plane, eye-mark to right and applied to right image, slight polarization, outer or right image being brighter

May 5th, 1859, 8 P M —Prismatic parhelion with part of halo on each side of sun distant about 22° 30′ Polariscope applied to left image, held in a parallel plane, eye-mark to left, *medium* polarization, left or outer image brighter Instrument held in same plane and applied to right image, eye-mark to right, similar *medium* polarization, outer or right image brighter

May 6th, 1859, 6 50 P M.—Prismatic parhelion and part of halo on each side of sun distant 22° 20′ Polariscope applied to left image and held in a parallel plane, eye-mark to left, *medium* polarization, outer or left image being brighter Instrument held in same plane, eye-mark to right, and applied to the right parhelion, similar *medium* polarization, right or outer image being brighter

May 20th, 1859, 8 P M —Prismatic arc of halo to left of sun distant about 23°. Polariscope applied, held in the parallel plane, eye-mark to left, slight polarization, the outer or left image being brighter.

August 7th, 1859, 7 30 P M —Prismatic parhelion to right of sun, distant about 22° 30′ Polariscope applied, held in the plane of a line joining sun and parhelion, a little more polarization than in last observation Arrow and brighter image both to the right

XIV "Notice of 'The Royal Charter Storm' in October 1859" By Rear-Admiral ROBERT FITZROY, F R.S. Received June 21, 1860

(Abstract)

The author commenced with some remarks on the recent progress of meteorology, on its advances towards precision and consistency as a science, and the comparative certainty and confidence with which it may now be relied on in its practical applications He adverted also to the measures now systematically adopted by the Meteorological Department of the Board of Trade and by the Admiralty for promoting simultaneous meteorological observations at various places, and for obtaining accurate registration of atmospherical conditions at sea and on land in many parts of the world, and drew attention to the steps taken by these public departments for diffusing practical knowledge concerning the laws of winds and storms, and of weather in general, among mariners and others more especially standing in need of such information, as well as to the practice now followed, of lending trustworthy meteorological instruments to commanders of ships able and willing to make an adequate return in the shape of accurate meteorological records, and of supplying other such aids, of a kind specially suited to their wants, to various of the most exposed and least affluent fishing villages

Notice was then taken of some of the more important practical results derived from meteorological inquiry,—especially the remarkable fact, now fully established, of the constancy of barometric pressure between five and ten degrees of north latitude, the phenomena of cyclones, and the explanation afforded of their production, following Dove's theory of polar and equatorial currents in the atmosphere.

In treating of the main subject of his communication, the "Royal Charter Storm" and the severe gale of the first two days of November last, the author made use of illustrative diagrams, which were exhibited to the Meeting Of these, four, of large size, showed successive phases of the storm on the 25th and 26th of October, the first at 9 A.M. and the second at 3 P.M. on the 25th, the third at 3 A.M and the fourth at 9 A M. on the 26th. These were intended to show simultaneous or synchronous direction and force of wind over a certain area within a few minutes of time, any noteworthy difference of longitude having been allowed for Smaller diagrams, in like manner, showed simultaneous direction and force of wind from the 22nd of October, to the 2nd of November. In both cases the direction was indicated with reference to the true meridian, and the force according to estimation only, which, however, was checked by many comparisons with velocities and pressure instrumentally obtained at the observatories of Greenwich, Cambridge, Oxford, Wrottesley, Liverpool, and at other well-known establishments. The same charts also showed curves of barometric pressure and curves of temperature These diagrams or charts were compiled by Mr. Babington, assisted in copying by Messrs. Pattrickson, Simmonds, and Symons; but notwithstanding the large amount of materials already made use of in their compilation, the author observed that none of them, nor indeed any part of the work, could yet be considered as nearly complete, and that much matter would still be added as, from time to time, information might be obtained from various sources.

The storm of the 26th of October was first noticed in accurate records, and measured by instrumental observations, in the Bay of Biscay, near Cape Finisterre in Spain This particular tempest did not come from the west, but from the south-south-west, true

Successive barometric effects of the storm were traced by similar means in that direction from S S W to N N E across England, from the Channel through Cornwall, across the central southern counties and Lincolnshire, over the North Sea to between the Shetland Isles and Norway

By referring to these charts and the diagrams, it will be seen that the lowest barometer and a corresponding or simultaneous lull prevailed over ten, fifteen, or twenty miles consecutively in the direction pointed out. But at the time that this comparative lull existed there was around this centre, by some called a vortex (but it can hardly be appropriately so termed, because there was no central disturbance), only variable wind or calm for a short time in the middle of the space, which was about ten or fifteen miles in irregular area

The wind obtained a varying velocity of from 60 to 100 miles an hour at a distance of from twenty to about fifty miles from this space, and in unequal eddyings crossed England towards the north-north-east, the wind blowing from all points of the compass around the lull. When at Anglesea the storm came from the north-north-east, in the Straits of Dover it was from the south-west, on the east coast it was easterly, in the Irish Channel it was northerly, and on the coast of Ireland it was from the north-west

The charts show that this circulation or cyclonic commotion was passing northwards from the 25th to the 27th, being two complete days from the time of its first great strength in the "Chops of the Channel," while outside of this circulation the wind became less and less violent; and it is very remarkable that even so near as on the west coast of Ireland there was fine weather with light winds, while in the British Channel it blew a furious northerly and westerly gale

At Galway and at Limerick on that occasion there were light winds only, while, as already stated, over England, the wind was passing in a tempest, blowing from all points of the compass around a central lull.

The next storm that occurred was similar in its features, though it came from a rather different direction

It raged on the 1st and 2nd of November, and its character was in all respects like that just described, now usually called the "Charter Storm"

Coming more from the westward, it passed across the north of Ireland, the Isle of Man, north of England, and then across the North Sea towards Denmark Further than that distance facts have not yet been gathered, but in the course of time they will be obtained and collated

The general effect of these storms was felt unequally in our islands, and much less inland than on the coasts

Lord Wrottesley has shown by observations made at his observatory in Staffordshire, that the wind is diminished or checked by its passage over land, and, looking to the mountain ranges of Wales and Scotland, rising 2000, 3000, or 4000 feet above the level of the ocean, we see they must have great power to alter the direction and probably the velocity of wind, independently of alterations caused by changes of temperature The very remarkable similarities of this storm of the 1st and 2nd of November, that of the 25th and 26th of October, the series of storms investigated by Dr. Lloyd during ten years, and the observations of Mr William Stevenson in Berwickshire, require special notice on this occasion. There is no discrepancy between the results of the ten years' investigations published by Dr. Lloyd in the Transactions of the Irish Academy, the three years' inquiry published by Mr William Stevenson, and all the investigations which have been brought together during the last four years. They all tell the same story Dr Lloyd only found in ten years one instance of even a partial storm which differed, namely, one that came from the north in the *first instance*

Storms from the south-west are followed by sudden and dangerous storms from the north or east, and these are the storms that do most damage on our coasts. Upon tracing the facts, it is proved that the storms which come from the west and south come on gradually, but that storms from the north or east begin suddenly, and at times with extraordinary force.

The barometer, with these north-eastern storms, does not give so much warning upon this coast, because it ranges higher than with the wind from the opposite quarter But though the barometer does not give much indication of a north-eastern gale, the thermometer does, and the now well-known average temperature of every week in the year affords the caution The temperature being much above or below the mean for the time of year shows whether the wind will be northerly or southerly—thanks to Mr Glaisher's discussion of the Greenwich observations for temperature

To revert to a few of the signs which preceded the "Charter Gale" For a few days before that storm came on, the thermometer was exceedingly low over all the country, there were north winds in some places, and a good deal of snow, though there had been a great deal of exceedingly dry and hot weather previously These anomalies require consideration, and it may be mentioned that everywhere in these islands, for days before that time, from the 22nd to the 25th of October, barometers were very low Many days preceding the Charter storm, an extraordinary clearness in the atmosphere was noticed in the north of Ireland, the mountains of Scotland were never seen so prominently as they were in the few days preceding those on which the great storm took place Every one is aware that last summer was remarkable for its warmth It was exceedingly dry and hot All over the world, not only in the Arctic, but in the Antarctic regions, in Australia, South America, in the West Indies, Bermudas, and elsewhere, auroras and meteors were unusually prevalent, and they were more remarkable in their features and appearances than had been noticed for many years There was also an extraordinary disturbance of currents along telegraph wires They were so disturbed at times, that it was evident there were great electric or magnetic storms in the atmosphere, though they could be traced to no apparent cause

Probably these electric disturbances were connected with a peculiar action of the sun upon our atmosphere. Submarine wires, as well as electrical wires above ground, were unusually disturbed, and these disturbances were followed within two or three days by great commotions in the atmosphere, or by some remarkable change.

The question of areas of barometric pressure—or lines (which Espy contends for), namely, long lines from north to south, or from one point direct to another, having been much discussed, the principal object of making the sections, as it were soundings, of the atmosphere which are shown in the diagrams, was to prove whether lines of pressure, or whether areas of pressure prevailed, and in the author's opinion, when they are closely examined they go to prove that while the atmosphere in the British Islands varies in its pressure from time to time, such variation is not along a particular line, but extended over a large and wide area

As remarkable exceptions to the force of these particular storms, it may be noted that at some places there was little or no wind, although the barometer fell much, without any consequence but rain The wind circulating around these districts did not affect them, while at other places the storm was tremendous

The following few details are given respecting the data on which the diagrams have been constructed —

The probable limits of error of the barometric curves on the synoptic charts, 21st October to 2nd November, 1859

1 *Observations quite correct.*—The observations at the regular observatories, such as Greenwich, Oxford, Cambridge, Wrottesley, Highfield House, Kew, &c , have had all corrections applied, and have been reduced to sea-level, and the temperature of 32° Fahrenheit.

2. *Error probably very small, less certainly than half a tenth.*—The returns from members of the British and Scottish Meteorological Society (nearly ninety in number) have been corrected for height above sea-level, within a few feet, and the corrections of instrumental errors with reductions to 32° have been applied.

Observations probably within a few hundredths of an inch.—The continental observations, collected from Dutch papers and from the "Moniteur," have been reduced to 32°, and have also been corrected for instrumental errors.

The heights of some stations are known, and the corrections due to those heights have been applied, while others are but little, if at all, above the sea-level.

Any error in laying down curves from these data can scarcely have exceeded two or three hundredths of an inch

3 *Observations less accurate* —The heights of the stations of some observers are not known so nearly. Other corrections have been applied only in a few cases,—the observations sometimes recorded only to the nearest tenth, as at a few lighthouses, not being deemed sufficiently reliable.

Returns in which the barometrical observations are evidently erroneous (from comparison with other neighbouring and contemporaneous observations) have been rejected

On the whole it may be safely assumed that the observations from which the curves are laid down are less than a tenth in error

4 *Lighthouses* —The heights of the lantern above the sea-level, and of the tower, being known, the heights of the barometers have been ascertained, and corrections for the heights have been applied

Comparisons of Wind Scales

Sea	Wind	Land
0 to 3	Light	0 to 1
3 to 5	Moderate	1 to 2
5 to 7	Fresh	2 to 3
7 to 8	Strong	3 to 4
8 to 10	Heavy	4 to 5
10 to 12	Violent	5 to 6

Wind Pressure lbs (avoirdupois)	No (land scale)	Velocity Miles (hourly).
½	1	10
5	2	32
10	3	45
21	4	65
26	5	72
32	6	80

These comparisons of scales have been used in the wind charts, and have been found convenient as well as sufficiently accurate.

XV "Contributions to the History of the Phosphorus Bases" Parts I., II. and III By A. W Hofmann, LL D., F.R S. &c. Received June 21, 1860.

This paper contains the first three sections of the full and systematic exposition of the author's Researches on the Phosphorus Bases, of which brief notices have from time to time been communicated by him to the Society, and printed in the 'Proceedings' The present communication comprehends

I Deportment of *Triethylphosphine with Sulphur compounds* Nitro-phosphuretted Ureas

II Theory of Diatomic Bases Diphosphonium Series

III Theory of Diatomic Bases Phosphammonium—and Phospharsonium Series

Communications received since the end of the Session

I "On Boric Ethide" By Edward Frankland, Ph D, F.R S, and B. Duppa, Esq Communicated by Dr Frankland Received July 7, 1860.

When zincethyl in excess is brought into contact with tribasic boracic ether, $\left(B\left\{\begin{matrix}C_4H_5O_2\\C_4H_5O_2\\C_4H_5O_2\end{matrix}\right.\right)$, the temperature of the mixture gradually rises for about half an hour If it be now submitted to distillation, it begins to boil at 94° C, and between this temperature and 140° a considerable quantity of a colourless liquid distils over The distillation then suddenly stops, the thermometer rises rapidly, and, to avoid secondary products of decomposition, the operation should now be interrupted. The materials remaining in the retort solidify, on cooling, into a mass of large crystals, which are a compound of ethylate of zinc with zincethyl. On rectification, the distillate began to boil at 70°, but the thermometer rapidly rose to 95°, at which temperature the last two-thirds of the liquid passed over and were received apart. The product thus collected exhibited a constant boiling-point on redistillation Submitted to analysis, it yielded results agreeing with the formula

$$B\left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right.$$

This body, for which we propose the name *boric ethide*, is produced by the following reaction —

$$\underbrace{2B\left\{\begin{matrix} C_4H_5O_2 \\ C_4H_5O_2 \\ C_4H_5O_2 \end{matrix}\right.}_{\text{Boracic ether}} + \underbrace{3Zn_2\left\{\begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}\right.}_{\text{Zincethyl}} = \underbrace{2B\left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right.}_{\text{Boric ethide}} + \underbrace{6\left.\begin{matrix} C_4H_5 \\ Zn \end{matrix}\right\}O_2}_{\text{Ethylate of zinc}}$$

The ethylate of zinc thus formed combines with zincethyl to form the crystalline compound above alluded to

Boric ethide possesses the following properties —It is a colourless mobile liquid of a pungent odour, its vapour is very irritating to the mucous membrane, and provokes a copious flow of tears The specific gravity of boric ethide at 23° C is 6961, it boils at 95° C, and the results of several determinations of its vapour-density give the number 3·4006 The calculated vapour-density of boric ethide, volumetrically composed like terchloride of boron, is 3·3824

Boric ethide is insoluble in water, and is very slowly decomposed by prolonged contact with it. Iodine has scarcely any action upon it, even at 100° C. It floats upon concentrated nitric acid for several minutes without change, but suddenly a violent oxidation takes place, and crystals of boracic acid separate. When boric ethide vapour comes in contact with the air it produces slight bluish-white fumes, which have a high temperature. The liquid is spontaneously inflammable in air, burning with a beautiful green and somewhat fuliginous flame In contact with pure oxygen it explodes Placed in a flask and allowed to oxidize gradually, first in dry air and finally in dry oxygen, it forms a colourless liquid, which boils at a higher temperature than boric ethide, but cannot be distilled under atmospheric pressure without partial decomposition In a stream of dry carbonic acid this product of oxidation evaporates without residue. By distillation *in vacuo* it is obtained pure, and it then exhibits a composition expressed by the formula

$$B\left\{\begin{matrix} C_4H_5 \\ C_4H_5O_2 \\ C_4H_5O_2 \end{matrix}\right.$$

The product of the oxidation of boric ethide is therefore the *diethylate* of a body which may be conveniently named *boric dioxyethide*, $\left(B\begin{cases}C_4H_5\\O\\O\end{cases}\right)$. The formation of diethylate of boric dioxyethide from boric ethide may be thus represented

$$\underbrace{B\begin{cases}C_4H_5\\C_4H_5\\C_4H_5\end{cases}}_{\text{Boric ethide}} + O_4 = \underbrace{B\begin{cases}C_4H_5\\C_4H_5O_2\\C_4H_5O_2\end{cases}}_{\text{Diethylate of boric dioxyethide}}$$

This compound dissolves instantly in water, and is resolved into alcohol and a volatile white crystalline body, which may be sublimed without change, at a gentle heat, in a stream of carbonic acid, and then condenses in magnificent crystalline plates like naphthaline The analytical results yielded by this body agree closely with the formula

$$B\begin{cases}C_4H_5\\HO_2\\HO_2\end{cases}$$

It is therefore obviously produced by the substitution of two atoms of hydrogen for two of ethyl in diethylate of boric dioxyethide

$$\underbrace{B\begin{cases}C_4H_5\\C_4H_5O_2\\C_4H_5O_2\end{cases}}_{\substack{\text{Diethylate of boric}\\\text{dioxyethide}}} + 2\left.\begin{matrix}H\\H\end{matrix}\right\}O_2 = \underbrace{B\begin{cases}C_4H_5\\HO_2\\HO_2\end{cases}}_{\substack{\text{Dihydrate of boric}\\\text{dioxyethide}}} + \underbrace{2\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}O_2}_{\text{Alcohol}}$$

Dihydrate of boric dioxyethide possesses an agreeable etherial odour and a most intensely sweet taste. Exposed to the air it evaporates slowly at ordinary temperatures, undergoing at the same time partial decomposition, and invariably leaving a slight residue of boracic acid. Its vapour tastes intensely sweet It reddens litmus paper, although in other respects its acid qualities are very obscure. It is very soluble in water, alcohol, and ether. Exposed to a gentle heat it fuses, and at a higher temperature boils with partial decomposition.

We are at present engaged with the further study of these bodies, and with the corresponding reactions of zincethyl upon silicic, carbonic, and oxalic ethers.

II. "On Fermat's Theorems of the Polygonal Numbers." First Communication. By The Right Hon. Sir Frederick Pollock, F.R.S., Lord Chief Baron. Received July 11, 1860.

(Abstract.)

This paper relates to the second theorem, viz. that which asserts that every number is composed of 4 square numbers (0 [or zero] being considered as an even square). If every odd number be composed of 4 square numbers, then every even number must also be composed of 4 square numbers; for every even number must, on a continued division by 2, ultimately become an odd number. The paper relates chiefly to the Table which accompanies it, from which it appears that a remarkable law obtains as to the division of odd numbers $(2n+1)$ into 4 square numbers—when a number of the form $4n+1$ is divisible into 2 square numbers, which (as $4n+1$ is an odd number) must be one of them odd, the other even. Before explaining the Table, it is proper to state that if an odd number be divisible into 4 square numbers, three of them must be odd, and one of them even, or one of them must be odd, and 3 of them even, otherwise their sum cannot be an odd number; it follows from this that if the difference between any two of them be an odd number, the difference between the other two must be an even number, and *vice versâ*, for let $a^2+b^2+c^2+d^2=2n+1$, then if $a^2-b^2=2p$, c^2-d^2 must equal $2q+1$; if possible let $c^2-d^2=2r$, then $a^2-b^2+c^2-d^2=2p+2r$, add $2b^2+2d^2$ (an even number) to each, and $a^2+b^2+c^2+d^2$ will be an even number, which by the hypothesis it is not; if, therefore, a^2-b^2 be an even number, c^2-d^2 cannot also be an even number, and therefore must be an odd one. If, therefore, the four roots of the squares into which any odd number may be divided are arranged in any order there will be three differences, the two exterior differences will be one odd, the other even, the middle difference may be either odd or even.

The Table is arranged thus:—the lowest row of figures is the series 1, 5, 9, 13, 17, &c. $(4n+1)$; the next row above is the series of natural numbers, 0, 1, 2, 3, 4, &c. (n), &c., the next row is 1, 3, 5, 7, 9, &c. $(2n+1)$ the odd numbers; each of the odd numbers is the first term in a series increasing upwards by the numbers 2, 4, 6, 8, 10, &c., forming an arithmetic series of the second order (the first and second differences being respectively 2 each), when the number in the lowest row cannot be divided into 2 squares, the arithmetic series is not formed, and the squares are marked with an asterisk, but when the number $4n+1$ is divisible into 2 square numbers, the roots of these squares constitute the two exterior differences of the roots into which the odd number may be divided, and also of the roots into which each term of the series increasing upward may be divided, the middle difference of the roots will be the smaller half of the sum of the 2 roots of the square numbers into which $4n+1$ may be divided, with a negative sign, and will increase by 1 in each successive term of the upward series

For example, in the Table take the number 29 in the lowest row, $7\times4+1=29$, 7 is the number above it, and $7\times2+1=15$ the odd number, which is the first term of the series 15, 17, 21, 27, 35, &c. Now 29 is composed of 2 square numbers, 4 and 25, whose roots are 2 and 5, $2+5=7$, the smaller half is 3, and 2, −3, 5 will be the differences of the roots of the squares into which 15 may be divided, and whose sum will equal 1, thus

2, −3, 5
−1, 1, −2, 3,

the roots when squared and added together equal 15, and the other terms of the series follow in like manner, obeying the law indicated, thus

5, −2, 2
3, 2, 0, 2 when squared and added =17
2, −1, 5
−2, 0, −1, 4 when squared and added . =21
5, 0, 2
−4, 1, 1, 3 when squared and added =27
2, 1, 5
−3, −1, 0, 5 whose squares . . . =35

The proof of all this depends on a property of numbers mentioned in the Philosophical Transactions for 1854, vol cxliv. p. 317.

If any number be composed of two triangular numbers, it will also equal a square and a double triangular number. If

$$n=\frac{a^2+a}{2}+\frac{b^2+b}{2},$$

it will be of the form a^2+a+b^2, and may be assumed equal to

a^2+a+b^2 For if 2 numbers be both odd or both even, they may always be represented by $a+b$ and $a-b$, if one be odd and the other even, they may always be represented by $a+b\pm1$ and $a-b$, or by $a+b$ and $a-b\pm1$, and if the two numbers be made the bases of trigonal numbers, the sum of the two trigonal numbers will always be of the form a^2+a+b^2, or a^2+b+b^2 now when any number in the natural series of numbers is composed of two triangular numbers, it may be represented by a^2+a+b^2, and $4n+1$ will then equal $4a^2+4a+1+4b^2$,—obviously the sum of an odd and an even square, whose roots are $2b$ and $2a+1$, and $2n+1$, the corresponding odd number, will equal $2a^2+2a+1+2b^2$,—obviously composed of 4 square numbers, whose roots are b, b, a, $a+1$, and if they be arranged thus,

$$\begin{array}{c} 2b,\ -(a+b)\ 2a+1 \\ -b,\ b,\ -a,\ a+1, \end{array}$$

so that the sum of their roots may equal 1, the exterior differences of the roots will be $2b$ and $2a+1$, the roots of the two squares into which $4n+1$ is divisible, and the middle difference will be $-(a+b)$, the smaller half of the sum of the roots $(2b+2a+1)$ with a negative sign, if the exterior differences be reversed and the middle difference be increased by 1, the differences will be $2a+1$, $-(a+b-1)$, $2b$, and the roots whose sum will equal 1 will be, with their differences above them,

$$\begin{array}{c} 2a+1,\ -(a+b-1),\ 2b \\ -(a+1),\ a-(b-1),\ b+1, \end{array}$$

and the sum of the squares of the roots will be 2 more, from these two sets of roots all the rest may be obtained, by adding one to each of two roots and subtracting 1 from each of the other two roots, the exterior differences of the roots will therefore always be the same, and the middle difference will increase by 1 at each step; the sum of the squares of the roots will increase by

2, 4, 6, 8, &c

As the sum of any two square numbers of which one is odd and the other even $(4a^2+4a+1+4b^2)$ must be of the form $4n+1$, every possible case of an odd square combined with an even square must occur somewhere in the series

1, 5, 9, 13, &c.,

and the Table (if extended) must contain every possible case of odd and even numbers as exterior differences, combined with every possible and available middle difference, for negative differences may be rejected, inasmuch as, if the roots be put according to their algebraic value, all the differences must be even, thus the roots and differences of 15 above were

$$\begin{matrix} 2, -3, 5 \\ -1, 1, -2, 3, \end{matrix}$$

if the roots be placed according to their algebraic value, they would be -2, -1, 1, 3, and with the differences above

$$\begin{matrix} 1, 2, 2 \\ -2, -1, 1, 3, \end{matrix}$$

15 will therefore be found in the column above 5, and in the fourth place. The Table (extended indefinitely) would therefore contain every possible odd number the sum of whose roots may equal 1.

It is possible that this connexion between the roots of the squares into which $4n+1$ may be divided, with the exterior differences of the roots of the four square numbers into which $2n+1$ may be divided, formed part of the mysterious properties of numbers to which Fermat alluded when he announced the theorems of the polygonal numbers

III. "On Cyanide of Ethylene and Succinic Acid."—Preliminary Notice. By Maxwell Simpson, Ph.D. Communicated by Dr Frankland, F R.S. Received August 1, 1860.

Succinic acid bears the same relation to the diatomic alcohol (glycol) that propionic acid bears to ordinary alcohol. Propionic acid can be obtained by treating the cyanide of the alcohol radical with potash. Can succinic acid be obtained by treating the cyanide of the glycol radical with the same reagent, or is it an isomeric acid that is formed under these circumstances?

$$\underset{\text{Cyanide of ethyl.}}{C_4H_5, Cy} + O_2\left\{\begin{matrix}K\\H\end{matrix}\right. + 2HO = \underset{\text{Propionate of potash.}}{O_2\left\{\begin{matrix}C_6H_5O_2\\K\end{matrix}\right.} + NH_3.$$

$$\underset{\text{Cyanide of ethylene.}}{C_4H_4\,2Cy} + 2\left(O_2\left\{\begin{matrix}K\\H\end{matrix}\right.\right) + 4\dot{H}O = \underset{\text{Succinate of potash?}}{O_4\left\{\begin{matrix}C_8H_4O_4''\\K_2\end{matrix}\right.} + 2NH_3.$$

The following experiments were performed with the view of determining this point —

Preparation of Cyanide of Ethylene.—As a preliminary step to the formation of succinic acid in this way, it became of course necessary to prepare the cyanide of ethylene. This body I obtained by submitting bromide of ethylene to the action of cyanide of potassium.

The process was thus conducted.—A mixture of two equivalents of the cyanide and one of the bromide was introduced into a large balloon, together with a considerable quantity of alcohol, sp gr 840, and exposed to the temperature of a water-bath, a Liebig's condenser having been previously attached to the balloon in such a manner as to prevent the alcohol from distilling off the reacting ingredients. As soon as all the cyanide of potassium had been converted into bromide, the alcohol was separated and distilled A semifluid residue was thus obtained, which was filtered at the temperature of 100° Cent On treating the filtrate with a saturated solution of chloride of calcium, a reddish oil rose to the surface, which was well washed with ether, and exposed for some time to the temperature of 140°, in order to remove any bromide of ethylene that might have escaped the solvent action of the ether This body proved, on analysis, to be cyanide of ethylene It was not, however, quite pure There are difficulties attending its complete purification which I have not yet overcome.

At the temperature of the air, cyanide of ethylene is a semisolid crystalline mass of a brownish colour It melts under 50° Cent It is very soluble in water and alcohol, and sparingly soluble in ether. It cannot be distilled Nevertheless it bears a tolerably high temperature without suffering much decomposition Heated with an alcoholic solution of potash, it gives off ammonia Treated with nitric acid, it forms a body which crystallizes from alcohol in long needles This and some other reactions I am at present engaged in studying.

Preparation of Succinic Acid—Bromide of ethylene and cyanide of potassium were made to react upon each other in the same manner as in the preparation of the cyanide of ethylene. As soon as the reaction was complete, the alcohol was separated from the bromide of potassium, some sticks of caustic potash were added to it, and the whole heated for several days by means of a water-bath Torrents of ammonia were given off on applying the heat As soon as the evolution of this gas had ceased, the alcohol was distilled off and the residue treated with a considerable excess of hydrochloric acid This was then heated gently as long as acid vapours continued to be evolved, digested with absolute alcohol, and filtered, and then the filtrate was evaporated to dryness. The dry mass thus obtained was treated several times with alcohol in a similar manner. The result of these repeated digestions was then dissolved in water, and a few drops of a solution of nitrate of silver were added to it, which occasioned a slight precipitate of chloride of silver. This was separated by filtration, and the filtrate was exactly neutralized with ammonia On adding excess of nitrate of silver to this, an abundant white precipitate was obtained, very soluble in nitric acid and ammonia This gave, on analysis, numbers agreeing very well with the composition of succinate of silver. The acid itself possessed also all the properties of succinic acid It sublimed on the application of heat, was soluble in water, alcohol, and ether, and gave, when neutralized, a reddish-brown precipitate with perchloride of iron Moreover, on digesting this precipitate with ammonia, an acid could be detected in the filtered liquor, which gave white precipitates with nitrate of silver, and with a mixture of chloride of barium and alcohol

Succinic acid *can* then be obtained from glycol in the same manner as propionic acid from ordinary alcohol, the bromide of ethylene, the point from which I started, being capable of derivation from the diatomic alcohol

I propose extending this investigation to some other hydrocarbons of the series $C_n H_n$, with the view of ascertaining whether or not the homologues of succinic acid can be obtained from these bodies by a similar process.

IV. "Results of Researches on the Electric Function of the Torpedo." By Professor Carlo Matteucci of Pisa. In a Letter to Dr. Sharpey, Sec R S. Received August 3, 1860.

(Extract.)

"It has hitherto been believed that the action of the electric organ of the Torpedo was momentary only;—that it becomes charged under the influence of nervous action and discharged immediately that action ceases, somewhat like soft iron under the influence of an electric current Such, however, is not the real state of the case. The electric organ is always charged It may be conclusively shown by experiment that the action of that organ never ceases, and that round the body of a Torpedo, and probably of every other electric fish, there is a continual circulation of electricity in the liquid medium in which the animal is immersed In fact, when the electric organ, or even a fragment of it, is removed from the living fish and placed between the ends of a galvanometer, the needle remains deflected at a constant angle for twenty or thirty hours, or even longer

"I must here explain that in electro-physiological experiments it is highly advantageous to employ, as extremities of the galvanometer, plates of amalgamated zinc immersed in a neutral saturated solution of sulphate of zinc. This arrangement, which can be worked with the greatest facility, gives a perfectly homogeneous circuit, leaving the needle at zero in an instrument of 24,000 coils, the liquid in contact with the animal part experimented on has the greatest possible conductibility while it does not act chemically on the tissue, and the apparatus is entirely free from secondary polarity

"To return to the Torpedo The electric organ, or a portion of it, detached from the fish and kept at the temperature of freezing, preserves its electromotive properties for four, six, or even eight days, and an organ which has been kept for twenty-four hours in a vessel surrounded with a frigorific mixture of ice and salt, is found to possess an electromotive power as great as that of the organ recently detached from the living fish. Thus the electric organ retains its functional activity long after both muscular and nervous excitability have been extinguished

"What then is the action of the nerves on this apparatus? Here again experiment affords a very distinct and conclusive answer. Detach the organ of a live torpedo and cut it into two equal portions, in such a way as to leave each half in connexion with one of the large nervous trunks, place the two halves on a plate of gutta percha, with electric couples opposed, that is, with the similar surfaces (say the dorsal) in contact; and connect the two free (ventral) surfaces with the extremities of the galvanometer. , There will usually be no deflection of the needle, or, at most, a very slight effect which will soon disappear Now, after having opened the circuit of the galvanometer, irritate the nerve of one of the segments, by pinching, by the interrupted electric current, or in any other way, or prick the piece itself with a needle The portion of organ thus stimulated will give several discharges in succession, and a rheoscopic frog's limb with its nerve applied to the part will each time be thrown into violent convulsions If, after this, the galvanometer be applied as before, there will be a very strong deflection in a direction answering to the segment stimulated This deviation endures for a short time, but gradually becomes less, so that in a few minutes the effect of the two segments is equal Stimulation now of the other segment will in like manner render its electricity predominant These alternations may be repeated several times, but naturally the effect becomes less and less marked

"Thus the electromotive apparatus becomes charged and acts independently of the influence of the nerves, but that influence renews and renders persistent the activity of the apparatus We know, moreover, that the discharge, which is only a state of temporary increased activity of the organ, is brought on by an act of the will in the live animal, or by the excitation of the nerves of the organ

"I shall not enter now into further details respecting my recent experiments on the Torpedo, but I venture to think that we have really made a step towards clearing up the theory of the animal electromotive apparatus. *The organ of the Torpedo does not, under the influence of the nerves, act as an induction apparatus, the operation seems more analogous to that of a 'secondary pile,' created, through the influence of the nerves, in each constituent cell of the organ*

"The case is very different in muscular action, the changes occurring in which are better understood now that we know the phenomena of muscular respiration. I do not here refer to the variation of the muscular current which takes place at the moment of contraction. In that case it would appear from experiment, as I lately showed, that there are indications of a current in an opposite direction, but the conditions of the animal structure in action are so complex that no inference can be drawn as to the intimate nature of the phenomenon. It is otherwise, however, in comparing muscles which have been left at rest with muscles which have been fatigued by repeated contraction Being still engaged in the investigation of this matter, I shall content myself now with mentioning one result of my inquiry, which I consider as well established, the result, in fact, of performing on muscles the same kind of experiment as the one above described on the organ of the Torpedo The experiment is as follows:—Having selected a series of muscles, entire or divided, which have been proved (by my method of opposed muscular piles) to be equal in electromotive power, subject a certain number of them to repeated stimulation, and then, by means of the method of opposed couples, compare the muscles which have been exercised with those which have been left at rest, and it will be found that the latter will manifest a much greater degree of electromotive power than the former The nervous excitation, which causes muscular contraction, developes heat, generates mechanical force and consumes chemical affinity, and as the electromotive apparatus of muscle operates through means of that affinity, it must get weakened, like a pile in which the acid has become weaker In the Torpedo, on the other hand, there is neither heat nor mechanical force produced, and the electromotive apparatus is set up again, as it were, through the influence of the nerves, after the manner of a secondary pile"

V "Natural History of the Purple of the Ancients." By M Lacaze Duthiers, Professor of Zoology in the Faculty of Sciences of Lille. Communicated by Professor Huxley Received March 22, 1860*

The purple dye so esteemed by the ancients has by turns excited the curiosity of naturalists and of historians The number of memoirs upon the subject is considerable, and they are to be found in almost all tongues However, in all these works, remarkable in many respects, and which cannot be analysed in this short notice, three deficiencies are to be noted regarding matters of very great moment in the history of this substance

What are, 1st, the producing organs? 2ndly, the nature? 3rdly, the natural primitive colour of the dye? It is difficult to give any answer to these three questions by means of the facts contained in existing memoirs. It is for the purpose of replying to them that I have undertaken the investigation, whose chief results I have the honour now to lay before the scientific world

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* Translation received August 22, 1860

The two genera *Murex* and *Purpura* have yielded the species observed. In very distant localities, as at Mahon in Minorca, *Murex brandaris, M trunculus,* and *Purpura hæmastoma* have furnished results which observations conducted at Boulogne on *Purpura lapillus,* at Pornic (Vendée) on the same species and *Murex erinaceus,* and at La Rochelle and L'Ile de Rhé, have confirmed At Marseilles, *Murex brandaris* has yielded altogether similar results, and this concordance of all the observations permits me to offer them with much confidence

What is the organ which produces the dye?

The analogy which some chemists imagine they have found between the colour of alloxan or of murexide and the purple of the Mollusca, has led them to misconceive the nature of the organ which produces the colouring matter. It is indubitable that uric acid treated with nitric acid gives a beautiful reddish purple colour when the residue is exposed to ammoniacal vapour, and this reaction furnishes a means of detecting the renal organ in mollusks. But from this circumstance no one could be justified in concluding that the purple dye was either the secretion of the kidney or the result of a modification of the urine

Careful dissection of the purpuriferous mollusca proves that the purple dye is secreted by a very limited portion of the mantle, which can in no way be confounded with the true renal organ, as which the organ of Bojanus is now generally regarded, the position and the structure of the purpuriferous organ are indeed totally different from those of the kidney

Small in extent, this part occupies very nearly the space bounded by the branchiæ and the rectum, beyond whose extremities it hardly extends anteriorly, while posteriorly it, at most, reaches the organ of Bojanus It forms neither a sac nor a reservoir, as it has been stated to do, and these phrases, as well as 'purpuriferous vein,' should be rejected, because the organ is simply extended over the surface.

Large elongated cells, placed perpendicularly side by side on the surface of the pallial cavity in the direction of its greatest diameter, compose its tissue They form about two or three layers, the most exterior of which, covered with vibratile cilia, presents the most developed cells Below lies a very rich capillary network, which distributes the blood coming from the organ of Bojanus and the neighbouring parts of the mantle to the branchiæ The cells, when they have reached maturity, fall into the pallial cavity, become endosmotically distended, burst, and mingle their contents with the other mucus which already existed there This independent and isolated shedding of the histological elements constitutes the secretion of the dye-stuff, which, it is obvious, is not produced by a compound gland, or indeed by any gland in the proper sense of the word, but by a glandular portion of the pallial surface. It is the granular but soluble matter contained in these cells which possesses singular properties, and constitutes the dye-stuff

The peculiar layer whose position has just been indicated is not special, anatomically speaking, to the two genera *Murex* and *Purpura*, and this is important if, in looking at the matter morphologically, a similar part of the surface of the mantle of most gasteropods appears to produce a substance of like histological character, but different in its properties. In the Aplysiæ and the Snails it is naturally coloured, whilst in *Turbo littoralis* and *Trochus cinereus* it is colourless, and undergoes no modification by the action of the solar rays.

Thus, then, it is incorrect to say, with some chemists, that, anatomically speaking, the purple dye-stuff is yielded by the kidneys of *Mollusca*.

Anatomical investigation has led to the recognition in the genera *Murex* and *Purpura* of a peculiar anal gland placed alongside the rectum, and opening by a terminal pore close to the anus. This gland, which does not seem to have been described hitherto, is in structure and the arborescent disposition of its secretory cæca, a well-defined gland, and by this very circumstance it is impossible to confound it with the purpuriferous organ.

Properties of the Purple Dye-stuff.—A very curious fact, known from all antiquity, since the very existence of the dye depends upon it, is the transformation of the dye-stuff by the action of the solar rays. In the living animal this substance is at first colourless, or more or less yellowish, exposed to the light of the sun, in a moist state it acquires a pure violet hue, in a word, it is photogenic.

The solar action causes the three simple colours to be developed successively, and in the following order, yellow, blue, and red Between these, the compound colours green and violet which result from their mixture, are obtained with the greatest distinctness if the action is slow But whilst the yellow disappears by prolonged action, a considerable amount of blue always remains; whence in nature the final red is never pure, so that the dye always inclines more or less to violet

These properties have been placed beyond doubt by the possibility of making photographs on silk and cambric, which exhibit a remarkable delicacy in detail, combined with great strength of tone

In a photograph obtained in this way, the different tints through which the dye-stuff passes before becoming violet are more or less to be seen, but the deep violet predominates, and represents the black of ordinary photographs

The changes in the colour of the purple dye-stuff are accompanied by the production of a very penetrating fœtid odour, similar to that of essence of garlic The evolution of this odour is as characteristic of the solar action as the changes of colour, a consideration of much importance when we desire to solve the problem to which I now turn—*What was the primitive colour of the purple stuffs of antiquity?*

At first sight this question seems to be easily answered, but when one seeks for a precise signification of the word "purple," one soon becomes embarrassed If we ask a painter, without telling him why,—Be so good as to paint the shade which you would give to a purple drapery in a historical painting—each painter to whom the request is made will give a different colour. This is the case because no one has an exact idea of the primitive colour, which has been gradually modified, and which has now become the red, almost scarlet, which many painters understand by the word purple. It is only by the interpretation of the phrases of the ancients, and comparing them with direct observations, that one arrives at a solution of the difficulty, which would appear to be of great use to art.

It is enough to remark that the purple colour exists only because it has been developed by the sun, in justification of the conclusion that the ancients must have been acquainted with this peculiarity, as also with that of the development of the characteristic fœtid odour. Pliny, moreover, speaks of both, and hence it cannot be doubted that the purple was produced formerly exactly as at present, unless we admit that the animals and their dye-stuff have changed, which would be an altogether gratuitous hypothesis The conclusion to which we are driven then is this the colour was produced formerly as at present, under the same conditions and with the same characters, so that it ought to have been similar to that which we now obtain.

In simple and natural experiments the violet has never failed to appear, while pure red has always been absent One is led to conclude, therefore, that the natural and unmodified purple of the ancients was violet, as it is now, for whoever discovered it must have made the experiment, as it has been so often repeated, on the sea-shore, by breaking a purpuriferous mollusk, and crushing its mantle on moist linen which is exposed to the sun

Pliny cites Cornelius Nepos, who states positively that at first the violet purple was esteemed, and the passages of Plato and of Aristotle, which relate to the colour, lead to the same conclusion However, it cannot be doubted that though the colour of purple stuffs was primitively violet, the requirements of taste and of fashion led to the variation of its shades. Thus some stuffs were dyed twice, to give them a richer and more vivid colour—the so-called 'purpurea dibapha' The mixture of species also contributed to modify the hues

Murex trunculus gives an almost blue shade. The fishermen of Port Mahon told me that it always yielded that colour, and especially that it would give a fixed and permanent colour On the contrary, *Purpura hæmastoma* (which they call 'cor de fel') was known to them as staining their linen very permanently and ineffaceably

It ought also to be recollected that when mineral colours replaced the animal matter of mollusks, the hue varied, and though the term 'purple' might be retained, it was easy to pass by degrees to the deep red which rises in the mind when we recollect the purple worn by cardinals.

Perhaps also the manipulations to which the molluscan dye-stuff may have been subjected by the dyers, and of whose nature we know nothing, approximated the purple to the red, which Pliny compares to that of coagulated blood.

But it remains none the less demonstrated, both by the passages from ancient authors and by experiment, that the ***primitive and natural colour of the purple was formerly, as now, violet***

Hence it would appear to be requisite for a painter to consider the epoch when the personages who are represented clothed in purple drapery lived, for the hue varied with the age. The properties of the purple dye-stuff also render intelligible one ground of the esteem in which the colour was held, for, developed by the influence of light, it could not fade, like the red of cochineal for example, but must always have remained beautiful, even in the luminous and dazzling atmosphere of Italy and the East.

It would be difficult, with the scanty materials we possess, to determine exactly the species employed by the ancients Without doubt Pliny has indicated the two genera ***Murex*** and ***Purpura*** of the moderns by the names ***Purpura*** and ***Buccinum*** It is probable that ***Murex trunculus*** and ***brandaris***, and ***Purpura hæmastoma***, were employed by the dyers, but it would be difficult to identify the different species indicated by Pliny. Zoological investigations, accompanied by experiments which are all simply and easily made, would perhaps lead to results more definite than can be obtained by the interpretation of passages, if one could carry them out on the shores of countries formerly famous for their purple—those of Tyre for example.

Fig. 1

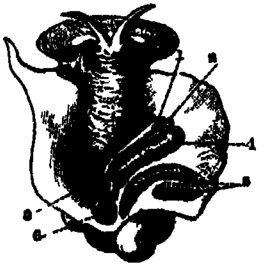

Fig 2

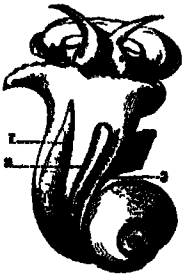

Fig 1 Animal with ***Purpura lapillus***, with the pallial cavity laid open
1 Genital orifice. 3 Anal gland. 5 Branchiæ.
2 Anus. 4 Purpurogenic organ 6 Organ of Bojanus.
Fig 2 The animal simply removed from its shell.
1 Branchiæ. 2 Purpurogenic organ 3 Anal gland

VI "Contributions towards the History of Azobenzol and Benzidine." By P W Hofmann, Ph.D. Communicated by Dr. Hofmann Received July 24, 1860

Among the numerous compounds into which benzol, when submitted to reagents, is converted, *azobenzol* and its derivatives have as yet received but limited attention Although more than twenty-five years have elapsed since this interesting body was discovered by Mitscherlich, both its formation and its constitution remain still doubtful.

Mitscherlich[*], who discovered azobenzol in 1834, when submitting nitrobenzol to the action of an alcoholic solution of potassa, represented this compound by the formula

$$C_6H_5N\dagger,$$

but left the reaction which gives rise to the formation of azobenzol unexplained. In 1845 this body was reprepared by Hofmann and Muspratt[‡], who observed among the collateral products of the reaction *aniline* and *oxalic acid* They represent the formation of azobenzol by the equation

$$\underbrace{2C_6H_5NO_2}_{\text{Nitrobenzol}}+\underbrace{C_2H_6O}_{\text{Alcohol}}=\underbrace{C_6H_5N}_{\text{Azobenzol}}+\underbrace{C_6H_7N}_{\text{Phenylamine}}+\underbrace{C_2H_2O_4}_{\text{Oxalic acid}}+H_2O,$$

adding at the same time that they are far from considering this equation as more than the representation of *one* phase of the transformation of nitrobenzol, since several other rather indefinite compounds or products are formed simultaneously.

At about the same period Zinin made the interesting observation that azobenzol is capable of fixing hydrogen and of being thereby converted into a well-defined base, benzidine, which he represented by the formula

$$C_6H_6N.$$

Considering the physical characters both of azobenzol and of benzidine, especially the high boiling-points of these substances, and the ratio of hydrogen and nitrogen in the latter compound, the sum of the number of equivalents of these two elements not being divisible by 2, many chemists were inclined to double the formulæ of both bodies, and to represent them by the following expressions —

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[*] Pogg. Ann xxxii p 224

[†] H=1, O=16, C=12, &c

[‡] Mem. of the Chem Soc vol. iii. p. 113

Azobenzol $C_{12}H_{10}N_2$
Benzidine $C_{12}H_{12}N_2$

This view received the first experimental confirmation in the formation of the nitro-derivatives of azobenzol, which were examined in 1849 by Gerhardt and Laurent The formation of

Nitrazobenzol $C_{12}H_9N_3O_2=C_{12}(H_9\ NO_2)\ N_2$, of
Dinitrazobenzol . $C_{12}H_8N_4O_4=C_{12}[H_8(NO_2)_2]N_2$,

and of several derivatives of these bodies, having established the C_{12}-formula of azobenzol, but little doubt could be entertained regarding the formula of benzidine, which is as readily obtained from azobenzol by reducing agents, as it may be reconverted into azobenzol by nitric acid*

The molecular value of benzidine being thus almost exclusively fixed by the determination of the formula of the compound from which it originates, it was of some interest to obtain additional experimental evidence for the molecular weight of azobenzol

With this view I have determined the vapour-density of azobenzol. This body boiling at a rather high temperature, I have availed myself of the method of displacement lately proposed by Professor Hofmann. Experiment proved the density of the azobenzol-vapour to be 94 referred to hydrogen as unity, or 6 50 referred to air. The theoretical vapour-density of azobenzol, assuming that one molecule of this compound furnishes, like the rest of well-examined substances, 2 vols. of vapour†, is $\frac{182}{2}=91$ referred to hydrogen, and 6 32 referred to air.

The determination of the vapour-density, then, plainly confirms the higher molecular weights proposed for azobenzol and for benzidine.

When determining the vapour-density of azobenzol, I had occasion to observe that, probably in consequence of a typographical error, the boiling-point of this compound is misstated in all the manuals which I could consult, and even in the original memoirs of Mitscherlich himself The boiling-point is stated to be 193° C., whilst it is in reality 293° C.

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* Noble, Journal of the Chem. Soc. vol. viii p. 293.

† $H_2O=2$ vols.

Benzidine, when expressed by the formula

$$C_{12}H_{12}N_2,$$

presents itself as a well-defined diacid diamine The molecular construction of the diatomic base remained to be decided

I have endeavoured to solve this problem by the process of ethylation, as yet the simplest and the best guide in determining questions of this kind Benzidine in the presence of alcohol is rapidly attacked by iodide of ethyl After two hours' digestion at 100° C in sealed tubes, the reaction is complete The solution on evaporation yields a crystalline iodide,

$$C_{16}H_{22}N_2I_2=C_{12}H_{12}(C_2H_5)_2N_2I_2,$$

from which ammonia separates a solid crystalline base very similar to benzidine This compound, which fuses at 65° C, and resolidifies at 60° C, is *diethylbenzidine* ·

$$C_{16}H_{20}N_2=C_{12}H_{10}(C_2H_5)_2N_2,$$

which forms well-crystallizable salts with the acids, and yields with dichloride of platinum a difficultly soluble crystalline platinum-salt containing

$$C_{16}H_{22}N_2Cl_2\ 2PtCl_2.$$

When diethylbenzidine is treated again with iodide of ethyl, the phenomena previously observed repeat themselves The iodide

$$C_{20}H_{30}N_2I_2=C_{12}H_{10}(C_2H_5)_4N_2I_2$$

is formed, which when decomposed by ammonia yields *tetrethylbenzidine*

$$C_{20}H_{28}N_2=C_{12}H_8(C_2H_5)_4N_2$$

Tetrethylbenzidine resembles the diethylated and the non-ethylated base It fuses at 85° C, resolidifying at 80° C., produces with the acids crystalline compounds, and furnishes with dichloride of platinum a platinum-salt of the formula

$$C_{20}H_{30}N_2Cl_2,\ 2PtCl_2$$

The further action of iodide of ethyl upon tetrethylbenzidine is extremely slow After 12 hours' digestion at 100° C only a very minute quantity of the base had been transformed into an iodide Iodide of methyl, on the other hand, acts with great energy An hour's digestion is sufficient to produce the final diammonium-compound

The iodide

$$C_{22}H_{34}N_2I_2=C_{12}H_8(C_2H_5)_4(CH_3)_2N_2I_2$$

is very difficultly soluble in absolute alcohol, but dissolves with facility in boiling water, from which it is deposited on cooling, in long beautiful needles The solution of this iodide is no longer precipitated by ammonia, but yields with oxide of silver a powerfully alkaline solution, exhibiting all the characters of the completely substituted ammonium- and diammonium-bases discovered by Professor Hofmann The solution of this dimethyl-tetrethylated base, which contains

$$C_{22}H_{36}N_2O_2 = \left.\begin{matrix} C_{12}H_8(C_2H_5)_4(CH_3)_2N_2 \\ H_2 \end{matrix}\right\} O_2,$$

is not further acted upon by either iodide of ethyl or methyl. With acids it forms a series of salts which are remarkable for the beauty with which they crystallize The platinum-salt is almost insoluble in water, but soluble with difficulty in concentrated boiling hydrochloric acid, crystallizing from this solution on cooling in beautiful needles This salt contains

$$C_{22}H_{34}N_2Cl_2, 2PtCl_2$$

The above experiments appear to establish the molecular construction of benzidine in a satisfactory manner This base is obviously a primary diamine, in which the molecular group $C_{12}H_8$, whatever its nature may be, functions as a diatomic radical A glance at the subjoined Table exhibits the construction of benzidine and of the several compounds which I have described.

Diamines

Benzidine	$\left.\begin{matrix} (C_{12}H_8)'' \\ H_2 \\ H_2 \end{matrix}\right\} N_2,$
Diethylated benzidine	$\left.\begin{matrix} (C_{12}H_8)'' \\ (C_2H_5)_2 \\ (H_2)_2 \end{matrix}\right\} N_2,$
Tetrethylated benzidine	$\left.\begin{matrix} (C_{12}H_8)'' \\ (C_2H_5)_2 \\ (C_2H_5)_2 \end{matrix}\right\} N_2$

Iodides of Diammoniums.

Primary .	$[(C_{12}H_8)''\ H_6\ N_2]''I_2,$
Secondary	$[(C_{12}H_8)''\ H_4\ (C_2H_5)_2N_2]''I_2,$
Tertiary .	$[(C_{12}H_8)''\ H_2\ (C_2H_5)_4N_2]''I_2,$
Quartary .	$[(C_{12}H_8)''(CH_3)_2(C_2H_5)_4N_2]''I_2.$

The experiments described in this note were performed in Professor Hofmann's laboratory.

VII. "On Bromphenylamine and Chlorphenylamine" By E. T. Mills Communicated by Dr. Hofmann Received July 24, 1860.

Nitrophenylamine, when prepared from dinitrobenzol (i e by the indirect method), differs in so many respects from the isomeric base which is obtained from phenyl-compounds (i. e by the direct method), that chemists have distinguished these two bodies as alpha- and beta-nitrophenylamine*—Bromphenylamine and chlorphenylamine have hitherto been produced only by the action of potash upon bromisatin and chorisatin, the indirect method, by which they were originally obtained by Dr Hofmann, it appeared therefore of some interest to ascertain whether the bodies generated directly from

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* The alpha-nitrophenylamine (nitraniline) was formed about sixteen years ago by Dr Muspratt and myself (Chem Soc Mem vol iii p 112), by the action of reducing agents on dinitrobenzol The beta-nitraniline was discovered by Arppe (Chem Soc vol viii p 175), who obtained this compound when distilling pyrotartronitrophenylamide with potash The two bases resemble each other in a remarkable manner, but there are differences in their physical and chemical characters which leave no doubt as to the fact of their having different constitutions I may here remark that I have repeated Arppe's experiments, the results of which I can confirm in every particular Since the phenyl-compound from which Arppe obtained his substance is accessible only with difficulty, I have endeavoured to nitronate a more easily procurable phenyl compound Acetylphenylamide may be used for this purpose with considerable advantage. A solution of the compound in cold fuming nitric acid yields, on the addition of water, a crystalline difficultly soluble precipitate, which is easily obtained pure by recrystallization This substance contains

$$C_8 H_8 N_2 O_4 = \left.\begin{array}{l} [C_6 (H_4 NO_2)] \\ (C_2 \; H_3 \; O_2) \\ \;\;\;\;\;\; H \end{array}\right\} N,$$

and yields, when heated with potassa, the beta-nitrophenylamine of Arppe with all its properties. I may here recall a former observation, which has now become perfectly intelligible. When studying the action of nitric acid upon melaniline, I found (Chem. Soc. Mem t i 305) that the dinitromelaniline, which is thus formed, essentially differs from the dinitromelaniline obtained by submitting nitrophenylamine (alpha-) to the action of chloride of cyanogen. The two nitro-bases, which are both expressed by the formula

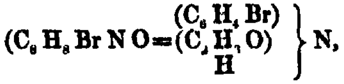
$$C_{13} H_{11} N_5 O_4 = C_{13} [H_{11} (NO_2)_2] N_3,$$

stand to each other in the same relation which obtains between alpha-nitrophenylamine and beta-nitrophenylamine *In fact, I have since found that the* distillation of the nitro-base, obtained by treating alpha-nitrophenylamine with chloride of cyanogen, furnishes alpha-nitrophenylamine; whilst beta-nitrophenylamine may be detected amongst the products of the distillation of the dinitromelaniline which is formed directly from melaniline by means of nitric acid —A. W H compounds of phenylamine would exhibit differences in their properties similar to those which distinguish alpha- and beta-nitrophenylamine.

With the view of deciding this question experimentally, I have submitted acetylphenylamide to the action of bromine and chlorine, in the hope of thus forming directly from phenylamine the brominated and chlorinated compounds in question

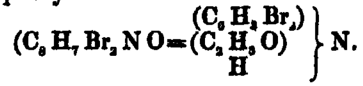
Action of Bromine on Acetylphenylamide

A cold aqueous solution of acetylphenylamide, when agitated with bromine gradually added in small quantities until the yellow colour imparted to the liquid no longer disappears, furnishes a crystalline compound difficultly soluble in cold, but easily recrystallizable from boiling water. The substance consists chiefly of monobrominated acetylphenylamide



$$(C_8 H_8 Br N O = \left.\begin{matrix}(C_6 H_4 Br)\\(C_2 H_3 O)\\H\end{matrix}\right\} N,$$

which is however invariably mixed with small quantities of dibrominated acetylphenylamide

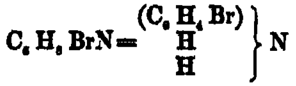


$$(C_8 H_7 Br_2 N O = \left.\begin{matrix}(C_6 H_3 Br_2)\\(C_2 H_3 O)\\H\end{matrix}\right\} N.$$

I have not been able to find a method of separating these two bodies perfectly.

The brominated compound is readily attacked by potash. On distilling the mixture, the vapour of water carries over a volatile body which solidifies in the condenser into beautiful acicular crystals, acetate of potassium remaining in the retort

The solidified distillate was purified by recrystallization from boiling water, and submitted to analysis. Both the combustion of the base itself and the platinum-determination of the beautiful golden-yellow platinum-salt proved this body to be bromphenylamine

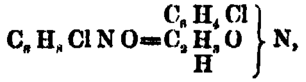


$$C_6 H_6 BrN = \left.\begin{matrix}(C_6 H_4 Br)\\H\\H\end{matrix}\right\} N$$

In its appearance, odour, and taste, as likewise in its deportment with acids and with solvents generally, the brominated base obtained from acetylbromophenylamide resembles perfectly the bromphenylamine produced from bromisatin, a specimen of which I obtained from Dr Hofmann's collection There is only one point in which a slight difference was observed Both compounds are capable of crystallizing either in needles or in well-defined octahedra, the former being generally obtained from water, and the latter from alcohol The bromphenylamine, obtained from the acetyl-compound, appears to be more inclined to crystallize in needles than in octahedra Circumstances have prevented me from entering into an examination of the products of decomposition of the two bromphenylamines, and the question whether these two bodies are really identical, or similarly related as the two nitro-compounds, must be decided by further experiments*

Action of Chlorine on Acetylphenylamide

The phenomena observed in the action of chlorine on a cold saturated solution of the phenyl-compound are perfectly similar to those presented in the corresponding reaction with bromine A crystalline compound immediately separates from the solution, as soon as the crystals cease to augment, the current of chlorine is interrupted Washed with cold, and once recrystallized from boiling water, the chlorinated body is found to be nearly perfectly pure monochlorinated acetylphenylamide



$$C_8H_8ClNO = \left.\begin{matrix} C_6H_4Cl \\ C_2H_3O \\ H \end{matrix}\right\} N,$$

which, when distilled with potash, furnishes abundance of chlorphenylamine, resembling in a marked manner the chlorphenylamine obtained by the action of potash upon chlorisatin.

VIII. "New Compounds produced by the substitution of Nitrogen for Hydrogen." By P Griess Communicated by Dr. Hofmann Received July 24, 1860.

In several previous notes I have called attention to a peculiar double acid which is formed by the action of nitrous acid upon amidobenzoic acid,

$$C_{14}H_{14}N_4O_4 + HNO_2 = C_{14}H_{11}N_5O_4 + 2H_2O†,$$

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[*] These experiments have since been made by Mr P Griess, whose results are given in the next abstract —A W H

[†] H=1, O=16, C=12, &c

the constitution of which, as far as my experiments go, may be represented by the formula

$$\left.\begin{matrix}[C_7(H_3 N_2{}')O][C_7(H_4, H_2N)O] \\ H_2\end{matrix}\right\} O_2.$$

There are not less than three other compounds known which empirically may be represented by the same formula as amidobenzoic acid, viz nitrotoluol, salicylamide, and anthranilic acid. The two former substances differ from amidobenzoic acid both physically and chemically in a marked manner; anthranilic acid, on the other hand, is so closely allied to the benzoic derivative, that special experiments were required to distinguish these two bodies Gerland, when he submitted the two acids to Piria's well-known reaction, observed that both are converted by nitrous acid into non-nitrogenated acids, which, although still isomeric, essentially differ in their properties, amidobenzoic acid being transformed into a new acid,—oxybenzoic acid, whilst anthranilic acid yields salicylic acid

It appeared of some interest to try whether the substitution of nitrogen for hydrogen in anthranilic acid would furnish a compound isomeric with the double acid obtained from amidobenzoic acid A current of nitrous acid, when passed into a cold alcoholic solution of anthranilic acid, rapidly transforms this substance into a compound crystallizing in white prisms, which is easily obtained by allowing the alcohol to evaporate at the common temperature The new body is extremely soluble in water, insoluble in ether. By analysis it was proved to contain

$$C_{14}H_9N_5O_7.$$

The new compound is thus seen to be far from isomeric with the derivative of amidobenzoic acid produced under similar circumstances, with which, in fact, it shows no analogy whatever. I have not yet arrived at a definite view regarding the molecular construction of this body, nevertheless its deportment with water shows even now that the nitrogen in it exists in two different forms. Gently heated with water, the new compound disengages torrents of nitrogen, on cooling, the liquid solidifies into a crystalline mass of salicylic acid, free nitric acid remaining in solution. This metamorphosis is represented by the equation

$$\underbrace{C_{14}H_9N_5O_7}_{\text{New body}}+2H_2O=N_4+HNO_3+2\underbrace{C_7H_6O_3}_{\text{Salicylic acid.}},$$

which has been controlled by quantitative experiments. The idea suggests itself to assume one-fifth of the nitrogen in the form of nitric acid, when the new body might be viewed as a salt like compound of the formula

$$\left.\begin{matrix} C_7 H_4 N_2' O_3 \\ C_7 H_4 N_2' O_3 \end{matrix}\right\} HNO_3 ,$$

the action of the water consisting simply in the replacement of the monatomic nitrogen by the elements of water, which would produce salicylic acid, nitric acid being liberated

I avail myself of this opportunity of mentioning the deportment of several other isomeric bodies under the influence of nitrous acid There are two basic compounds,

$$C_6 (H_6 NO_2) N,$$

known, the one is the alphaphenylamine of Hofmann and Muspratt, the other the betaphenylamine observed by Arppe When submitted to the action of nitrous acid, these two isomeric bodies yield two perfectly different nitrogen-substituted derivatives The substance obtained from alphaphenylamine (the base formed by the reduction of dinitrobenzol) has been already mentioned in one of my previous notes, the body derived from betaphenylamine is still under examination

The action of nitrous acid proves that there are also two bromphenylamines similar to the two nitrophenylamines The original bromphenylamine discovered by Hofmann, and which is formed by the distillation of bromisatin with hydrated potash, yields with nitrous acid a compound,

$$C_{12} H_9 Br_2 N_3 = \left.\begin{matrix} (C_6 H_4 Br)_2 \\ N''' \\ H \end{matrix}\right\} N_2,$$

crystallizing in beautiful golden-yellow needles, insoluble in water, and difficultly soluble in alcohol and ether The bromphenylamine, on the other hand, which was lately prepared by Mills* from acetylbromphenylamide, exhibits with nitrous acid a perfectly different deportment, being transformed into a yellow scarcely crystalline compound, easily soluble in alcohol and ether, but insoluble in water. I have not as yet analysed this compound, its formation, however, and its properties render it probable that it will be found to be isomeric with the product of decomposition previously mentioned I am engaged in a more minute examination of this compound, which I hope may assist in explaining the cause of the still enigmatical isomerism exhibited by the derivatives of phenylamine

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* See the previous abstract

I have already repeatedly called attention to the different atomicity exhibited by nitrogen under different conditions In the derivatives of amidobenzoic and of anthranilic acids, it can be proved that 1 equiv. of nitrogen replaces 1 equiv of hydrogen, while in the derivatives of phenylamine, the nitrogen is present with the value of three molecules of hydrogen

The experiments which I have described were performed in Dr. Hofmann's laboratory

IX "Contributions towards the History of the Monammines."—No. III Compound Ammonias by Inverse Substitution. By A W Hofmann, LL.D, F.R S, &c. Received July 24, 1860.

Many years ago I showed that the bromide or iodide of a quartary ammonium splits under the influence of heat into the bromide or iodide of an alcohol-radical on the one hand, and a tertiary monamine on the other

Having lately returned to the study of this class of substances, I was led to examine the deportment, under the influence of heat, of the tertiary, secondary, and, lastly, of the primary monammonium-salts

Experiment has shown that these substances undergo an analogous *decomposition*. The chloride *of a tertiary monammonium* when submitted to distillation yields, together with the chloride of an alcohol-radical, a secondary monamine, the chloride of a secondary monammonium, together with an alcohol-chloride, a primary monamine, lastly, the chloride of a primary monammonium, the chloride of an alcohol-radical and ammonia

Exactly, then, as my former experiments show that we may rise in the scale by replacing the four equivalents of hydrogen in ammonium one by one by radicals, so it is obvious from these new experiments that we may also step by step descend, by substituting again hydrogen for the radicals in succession

To take as an illustration the monammonium-salts of the ethyl-series which as yet I have chiefly examined

Ascent

$$\underbrace{H_3N}_{\text{Ammonia}} + \underbrace{(C_2H_5)Br}_{\text{Bromide of ethyl}} = \underbrace{[(C_2H_5)H_3N]Br}_{\text{Bromide of Ethylammonium}}$$

$$\underbrace{(C_2H_5)H_2N}_{\text{Ethylamine}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_2H_2N]Br}_{\text{Bromide of Diethylammonium}}$$

$$\underbrace{(C_2H_5)_2HN}_{\text{Diethylamine}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_3HN]Br}_{\text{Bromide of Triethylammonium}}$$

$$\underbrace{(C_2H_5)_3N}_{\text{Triethylamine}} + (C_2H_5)Br = \underbrace{[(C_2H_5)_4N]Br}_{\text{Bromide of Tetrethylammonium}}$$

Note —$H=1$, $C=12$

Descent

$$\underbrace{[(C_2H_5)_4N]Cl}_{\text{Chloride of Tetrethylammonium}} = \underbrace{(C_2H_5)Cl}_{\text{Chloride of Ethyl}} + \underbrace{(C_2H_5)_3N}_{\text{Triethylamine.}}$$

$$\underbrace{[(C_2H_5)_3HN]Cl}_{\text{Chloride of Triethylammonium}} = (C_2H_5)Cl + \underbrace{(C_2H_5)_2HN}_{\text{Diethylamine.}}$$

$$\underbrace{[(C_2H_5)_2H_2N]Cl}_{\text{Chloride of Diethylammonium}} = (C_2H_5)Cl + \underbrace{(C_2H_5)H_2N}_{\text{Ethylamine}}$$

$$\underbrace{[(C_2H_5)H_3N]Cl}_{\text{Chloride of Ethylammonium}} = (C_2H_5)Cl + \underbrace{H_3N}_{\text{Ammonia}}$$

The above reactions, interesting when regarded from a scientific point of view, admit of but limited application in practice. The purity of the result is disturbed by several circumstances, which it is difficult to exclude. Unless the temperature be sufficiently high, a small portion of the ammonium-salt submitted to distillation sublimes without change; again, a portion of the same salt is reproduced in the neck of the retort and in the receiver*, from the very constituents into which it splits, lastly, if the temperature be too high, the chloride of ethyl is apt to be decomposed into ethylene and hydrochloric acid, the latter producing, with the monamine liberated in the reaction, a salt which in its turn is likewise decomposed.

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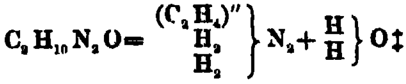
* This inconvenience may be partly obviated by distilling into an acid.

Thus the chloride of diethylammonium, for instance, together with chloride of ethyl and ethylamine, yields ethylene and chloride of ethylammonium which splits into chloride of ethyl and ammonia.

The idea naturally suggested itself, to attempt, by means of this reaction, the formation of the primary and secondary monophosphines, which are at present unknown. Experiments made with the view of transforming triethylphosphine into diethylphosphine have as yet remained unsuccessful, the chloride of triethylphosphonium distilling without alteration.

X. "Notes of Researches on the Polyammonias."—No. IX. Remarks on *anomalous* Vapour-densities. By A. W. Hofmann, LL.D., F.R.S. Received July 24, 1860.

In a note addressed to the Royal Society[*] at the commencement of this year, I have shown that the molecules of the diamines, like those of all other well-examined compounds, correspond to two volumes of vapour[†], and I have endeavoured to explain the apparent anomalous vapour-densities of the hydrated diamines by assuming that the vapour-volume experimentally obtained was a mixture of the vapour of the anhydrous base and of the vapour of water. Thus, hydrated ethylene-diamine was assumed to split under the influence of heat into anhydrous ethylene-diamine (2 vols. of vapour) and water (2 vols. of vapour).



$$C_2H_{10}N_2O = \left.\begin{matrix}(C_2H_4)''\\H_2\\H_2\end{matrix}\right\}N_2 + \left.\begin{matrix}H\\H\end{matrix}\right\}O\ [‡]$$

The vapour-density of ethylene-diamine referred to hydrogen being 30, and that of water vapour 9, the vapour-density of a mixture of equal volumes of ethylene-diamine and water-vapour $= \frac{30+9}{2} = 19.5$, which closely agrees with the result of experiment.

In continuing the study of the diamines, I have expanded these experiments Without going into the detail of the inquiry, I beg leave to record an observation which appears to furnish an experimental solution to the question

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[*] Proceedings, vol. x. p. 224.

[†] $H_2O = 2$ vols.

[‡] $H=1$, $O=16$, $C=12$, &c.

Ethylene-diamine, when submitted to the action of iodide of ethyl, yields a series of ethylated derivatives, amongst which the diethylated compound has claimed my particular attention This body in the anhydrous state is an oily liquid containing

$$C_6H_{16}N_2 = \left.\begin{array}{c}(C_2H_4)'' \\ (C_2H_5)_2 \\ H_2\end{array}\right\} N_2$$

With water it forms a beautiful crystalline very stable hydrate*, of the composition

$$C_6H_{18}N_2O = \left.\begin{array}{c}(C_2H_4)'' \\ (C_2H_5)_2 \\ H_2\end{array}\right\} N_2 + \left.\begin{array}{c}H \\ H\end{array}\right\} O$$

The vapour density of the anhydrous base was found by experiment to be 57 61, showing that the molecule of diethyl-ethylene-diamine corresponds to 2 vols of vapour, the theoretical density being $\frac{114}{2} = 57$

On submitting the crystalline hydrate to experiment, I arrived at the vapour-density 33 2 This number is in perfect accordance with the result obtained in the case of ethylene-diamine The legitimate interpretation of this number is that here again the hydrated base splits into the anhydrous diamine and water, and that the density observed is that of a mixture of equal volumes of diamine-vapour and of water-vapour, the theoretical density of which is $\frac{57+9}{2} = 33$ The correctness of this interpretation admits of an elegant experimental demonstration

Having observed that the hydrate loses its water when repeatedly distilled with a large excess of anhydrous baryta, the idea suggested itself, to attempt the decomposition of the hydrate in the state of *vapour* If the vapour obtained by heating this hydrate to a temperature 15° or 20° higher than its boiling-point actually consisted of a mixture of equal volumes of its two proximate constituents in a state of *dissociation* (to use a happy term proposed by Deville), it appeared very probable that the volume would be halved by the introduction of anhydrous baryta Experiment has verified this anticipation

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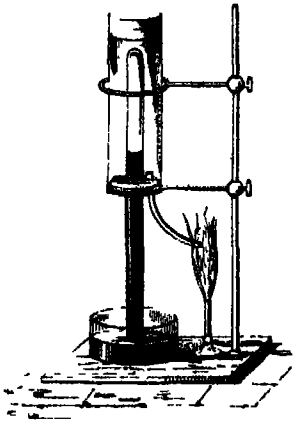
* Proceedings, vol x p. 104.



The upper half of a glass tube filled with, and inverted over, mercury, was surrounded by a second glass tube open at both ends and of a diameter about treble that of the former, the annular space between the two being closed at the bottom of the outer tube by a well-fitting cork The vessel thus formed round the upper part of the inner tube was moreover provided with a small bent copper tube open at the top and closed at the bottom, which was likewise fixed in the cork The vessel being filled with paraffin and a lamp being applied to the copper tube, the upper part of the mercury-tube could be conveniently kept at a high and constant temperature, whilst the lower end, immersed in the mercury-trough, remained accessible. A glance at the figure explains the disposition of the apparatus A small quantity of the hydrated base was then allowed to rise on the top of the mercury in the tube, and the paraffin bath having been heated to 170°, the volume of the vapour was observed Several pellets of anhydrous baryta were then allowed to ascend into the vapour-volume, while the temperature was maintained constant The mercury began immediately to rise, becoming stationary again, when a fraction of the vapour had disappeared, which amounted, the necessary corrections being made, to half the original volume

XI "Notes of Researches on the Poly-Ammonias."—No. X On Sulphamidobenzamine, a new base; and some Remarks upon Ureas and so-called Ureas. By A. W. Hofmann, LL.D, F.R.S. Received July 24, 1860

Among the numerous compounds capable of the metamorphosis involved in Zinin's beautiful reaction, the nitriles have hitherto escaped the attention of chemists This is the more remarkable, since some of these bodies are easily converted into crystalline nitro-compounds.

When examining several of the diamines which I have lately submitted to the Royal Society[*], I was induced to study the transformation which benzonitrile undergoes under the successive influence of nitric acid and reducing agents

Benzonitrile, when treated with a mixture of sulphuric and fuming nitric acid, furnishes, as is well known, a solid nitro-substitute which crystallizes from alcohol in beautiful white needles, containing

$$C_7 H_4 N_2 O_2 = C_7 (H_4, NO_2) N$$[†]

In order to obtain this body, it is desirable to perform the operation with small quantities, and to cool the liquid carefully, otherwise the formation of appreciable proportions of nitrobenzoic acid can scarcely be avoided

The nitro-compound is readily attacked by an aqueous solution of sulphide of ammonium, sulphur is abundantly precipitated, and on evaporating the liquid, a yellowish red oil is separated, which gradually and imperfectly solidifies. This substance possesses the characters of a weak base, dissolving with facility in acids, and being again precipitated by the addition of ammonia and the alkalies The preparation in the state of purity, both of the base itself and of its compounds, presents some difficulty This circumstance has prevented me from analysing the base I have, however, examined one of its products of decomposition, which leaves no doubt that nitrobenzonitrile, under the influence of reducing agents, undergoes the well-known transformation of nitro-compounds, and that the composition of the oily base is represented by the formula

$$C_7 H_6 N_2 = C_7 H_4 (NH_2) N$$

The oily base, when left in contact with sulphide of ammonium, is gradually changed, a crystalline compound being formed, which is easily soluble in alcohol and in ether, but difficultly soluble in water, and which may be purified by several crystallizations from boiling water, being deposited on cooling in white brilliant needles. This compound is a well-defined organic base, it dissolves with facility in acids, and is precipitated from these solutions by the addition

[*] Proceedings, vol x p. 104

[†] $H=1$; $O=16$, $C=12$, &c

of potassa or of ammonia. With hydrochloric acid it forms a crystallizable salt, which yields, with dichloride of platinum, an orange-yellow crystalline precipitate

On analysis, the new base was found to have the composition

$$C_7H_8N_2S,$$

explaining its formation, in which evidently two phases have to be distinguished·

$$(1)\quad C_7(H_4NO_2)N+3H_2S=2H_2O+3S+C_7H_6N_2,$$

$$(2)\quad C_7H_6N_2+H_2S=C_7H_8N_2S$$

The new sulphuretted base has the same composition as sulphocarbonyl-phenyldiamide, a feebly basic compound which I obtained some time ago by the action of ammonia on sulphocyanide of phenyl*.

$$C_7H_5NS+H_3N=C_7H_8N_2S$$

A superficial comparison of the properties of the two bodies shows, however, that they are only isomeric, the constitution of the latter compound being represented by the expression

$$\left.\begin{array}{c}(CS)''\\(C_6H_5)'\\H_3\end{array}\right\}N_2,$$

whilst the constitution of the former may be expressed by the formula

$$\left.\begin{array}{c}C_7(H_4H_2N)S\\H\\H\end{array}\right\}N=\left.\begin{array}{c}(C_7H_4S)''\\H_2\\H_2\end{array}\right\}N_2$$

The new sulphuretted base is closely connected with an interesting compound which Chancel obtained some years ago, when he submitted nitrobenzamide to the action of reducing agents. The crystalline base produced in this reaction contains

$$C_7H_8N_2O,$$

and differs from the body which forms the subject of this note only by having oxygen in the place of sulphur

The formation of this oxygenated compound has given rise to some misconceptions, which I take this opportunity to elucidate A short time before the discovery of the body in question, I had obtained a compound of exactly the same composition by the action of the vapour of cyanic acid upon aniline,

$$C_6H_7N+CHNO=C_7H_8N_2O.$$

The mode of producing this substance pointed it out as an analogue of urea, and hence the designation *aniline-urea,* under which I described the new body as the first of the group of compound ureas, which has since been so remarkably enriched by Wurtz and several other chemists

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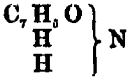
* Proceedings, vol. ix. p 276

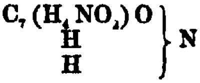
The aniline-urea, or phenyl-urea as it is more appropriately called, differs from ordinary urea in its deportment with acids, being, in fact, no longer capable of producing saline compounds The absence of basic properties in the new phenyl-compound was sufficient to throw some doubt upon its ureic character, and this doubt appeared to receive additional support by Chancel's subsequent discovery of a compound possessing not only the composition of phenyl-urea, but forming likewise well-defined saline combinations This compound is, however, the amide of amidobenzoic acid, its constitution being interpreted by Chancel, in accordance with its formation

Benzamide



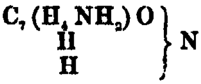
$$\left.\begin{matrix} C_7H_5O \\ H \\ H \end{matrix}\right\} N$$

Nitrobenzamide



$$\left.\begin{matrix} C_7(H_4NO_2)O \\ H \\ H \end{matrix}\right\} N$$

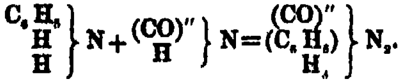
Amidobenzamide



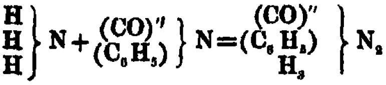
$$\left.\begin{matrix} C_7(H_4NH_2)O \\ H \\ H \end{matrix}\right\} N$$

Nevertheless chemists, by silent but general consent, began to look upon this compound as the *true* phenyl-urea, and in most manuals, even Gerhardt's 'Traité de Chimie' not excepted, it figures under this appellation

Let us see how far this view is supported by the deportment of this substance. Compound ureas, as I conceive the character of this class, must imitate the deportment of urea *par excellence,* both in their mode of formation and their products of decomposition Urea is formed whenever cyanic acid or cyanates come in contact with ammonia or ammoniacal salts. These are precisely the conditions under which the substance which I have described as phenyl-urea is generated. This compound is obtained by the union of cyanic acid with phenylamine, or of ammonia with cyanate of phenyl



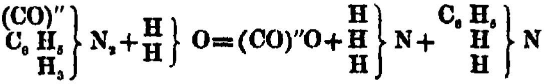
$$\left.\begin{matrix} C_6H_5 \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} (CO)'' \\ H \end{matrix}\right\} N = \left.\begin{matrix} (CO)'' \\ (C_6H_5) \\ H_3 \end{matrix}\right\} N_2.$$



$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}(CO)''\\(C_6H_5)\end{matrix}\right\}N=\left.\begin{matrix}(CO)''\\(C_6H_5)\\H_3\end{matrix}\right\}N_2$$

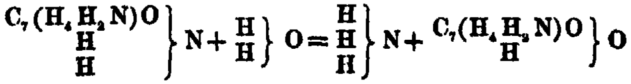
On the other hand, no cyanogen-compound is involved in the formation of amidobenzamide, or amidobenzamine, as it might be more appropriately called, on account of its basic properties.

Not less decisive is the evidence furnished by the products of decomposition of the two bodies The most characteristic transformation of urea is its decomposition into ammonia and carbonic acid when it is submitted to the action of the alkalies A compound urea thus treated should yield, together with carbonic acid and ammonia, the monamine from which it has arisen Phenyl-urea should furnish carbonic acid, ammonia, and phenylamine . these are precisely the products observed in the decomposition of the compound which is formed by the action of cyanic acid on phenylamine

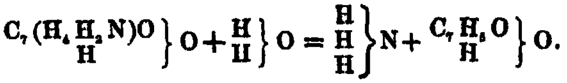


$$\left.\begin{matrix}(CO)''\\C_6H_5\\H_3\end{matrix}\right\}N_2+\left.\begin{matrix}H\\H\end{matrix}\right\}O=(CO)''O+\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N$$

Amidobenzamine, on the other hand, exhibits with potassa the deportment of an amidated amide. The reaction presents two distinct phases, ammonia and amidobenzoic acid being formed in the first phase, and ammonia and benzoic acid in the second



$$\left.\begin{matrix}C_7(H_4H_2N)O\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}H\\H\end{matrix}\right\}O=\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}C_7(H_4H_2N)O\\H\end{matrix}\right\}O$$



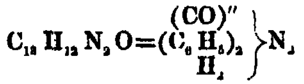
$$\left.\begin{matrix}C_7(H_4H_2N)O\\H\end{matrix}\right\}O+\left.\begin{matrix}H\\H\end{matrix}\right\}O=\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}C_7H_5O\\H\end{matrix}\right\}O.$$

No trace of carbonic acid and no trace of phenylamine are eliminated by potassa It is only by fusing with soda-lime that a perfect destruction of the compound ensues, when, as Chancel has distinctly observed, in the first place ammonia, and ultimately carbonic acid and phenylamine are evolved.

What I have said respecting phenyl-urea applies with equal force to diphenyl-urea. Gerhardt describes as diphenyl-urea the compound obtained by Laurent and Chancel when they examined the action of reducing agents upon nitrobenzophenone, and which, on account of its yellow colour, was originally described as *flavine*. This body contains $C_{18}H_{12}N_2O$, which is certainly the formula of diphenyl-urea But here again chemists have been misled by the basic properties of the substance

It is not my object at present to dwell on the constitution of flavine, which I intend to examine in a subsequent note, suffice it to say that this substance is not diphenyl-urea

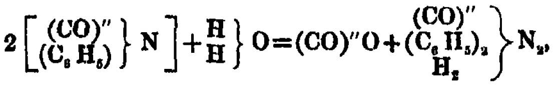
The true diphenyl-urea is the substance commonly called carbanilide, or carbophenylamide



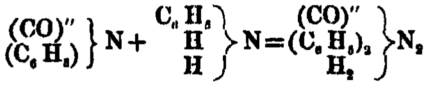
$$C_{13}H_{12}N_2O=\left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2$$

Both the conditions under which this body forms, and the products into which it is decomposed, leave no doubt regarding its position in the system.

This compound is formed by the action of cyanate of phenyl upon either water or phenylamine

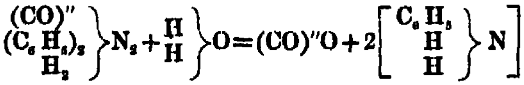


$$2\left[\left.\begin{matrix}(CO)''\\(C_6H_5)\end{matrix}\right\}N\right]+\left.\begin{matrix}H\\H\end{matrix}\right\}O=(CO)''O+\left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2,$$



$$\left.\begin{matrix}(CO)''\\(C_6H_5)\end{matrix}\right\}N+\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N=\left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2$$

When boiled with potassa, it splits into carbonic acid and phenylamine



$$\left.\begin{matrix}(CO)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2+\left.\begin{matrix}H\\H\end{matrix}\right\}O=(CO)''O+2\left[\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N\right]$$

These are the characters of *true* diphenyl-urea

XII "Researches on the Phosphorus-Bases"—No VIII. Oxide of Triethylphosphine. By A. W Hofmann, LL.D., F R S Received July 24, 1860.

In our former experiments *, Cahours and myself had often observed this substance, but we did not succeed in obtaining it in a state of purity fit for analysis Nevertheless, founding our conclusion on the composition of the corresponding sulphur-compound, and having regard to the analogies presented by the corresponding terms of the arsenic- and antimony-series, we designated this body as the oxide of the phosphorus-base

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* Phil Trans 1857, p 575

$$C_6 H_{15} PO = (C_2 H_5)_3 PO$$ *

I have since confirmed this formula by actual analysis

The difficulties which in our former experiments opposed the preparation of this compound in the pure state, arose entirely from the comparatively small quantity of material with which we had to work Nothing is easier than to obtain the oxide in a state of purity, provided the available quantity of material is sufficient for distillation In the course of a number of preparations of triethylphosphine for new experiments, a considerable quantity of the oxide had accumulated in the residues left after distilling the zinc-chloride compound with potash On subjecting these residues to distillation in a copper retort, a considerable quantity of the oxide passed over with the aqueous vapours, and a further quantity was obtained, as a tolerably anhydrous but strongly coloured liquid, by dry distillation of the solid cake of salts which remained after all the water had passed over. The watery distillate was evaporated on the water-bath as far as practicable, with or without addition of hydrochloric acid, and the concentrated solution was mixed with solid hydrate of potassium, which immediately separated the oxide in the form of an oily layer floating on the surface of the potash The united products were then left in contact with solid potash for twenty-four hours and again distilled The first portion of the distillate still contained traces of water and a thin superficial layer of triethylphosphine As soon as the distillate solidified, the receiver was charged, and the remaining portion—about nine-tenths—collected separately as the pure product To prevent absorption of water, the quantity required for analysis was taken during the distillation.

With reference to the properties of oxide of triethylphosphine, I may add the following statements to the description formerly given†. This substance crystallizes in beautiful needles, which, if an appreciable quantity of the fused compound be allowed to cool slowly, frequently acquire the length of several inches. I have been unable to obtain well-formed crystals, as yet I have not found a solvent from which

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* $H = 1$; $O = 16$, $C = 12$, &c

† Phil. Trans 1857, p 575.

this substance could be crystallized It is soluble in all proportions, both in water and alcohol, and separates from these solvents on evaporation in the liquid condition, solidifying only after every trace of water or alcohol is expelled Addition of ether to the alcoholic solution precipitates this body likewise as a liquid The fusing-point of oxide of triethylphosphine is 44°, the point of solidification at the same temperature. It boils at 240° (corr.)

As no determination of the vapour-density of any member of the group of compounds to which oxide of triethylphosphine belongs has yet been made, it appeared to me of some interest to perform this experiment with the oxide in question. As the quantity of material at my disposal was scarcely sufficient for the determination by Dumas's method, and Gay-Lussac's was inapplicable on account of the high boiling-point of the compound, I adopted a modification of the latter, consisting essentially in generating the vapour in the closed arm of a U-shaped tube immersed in a copper vessel containing heated paraffin, and calculating its volume from the weight of the mercury driven out of the other arm Since I intend to publish a full description of this method, which promises to be very useful in certain cases, I shall here content myself with stating the results obtained in one of the experiments

Substance .	0·150 grm
Volume of vapour	49·1 cub cent.
Temperature (corrected) .	266·6
Barometer at 0° ..	0·7670 metre.
Additional mercury column at 0°	0·1056 „

These numbers prove the vapour-density of oxide of triethylphosphine to be 66·30, referred to hydrogen as unity, or 4·60 referred to atmospheric air. Assuming that the molecule of oxide of triethylphosphine corresponds to 2 volumes of vapour*, the spec. grav. of its vapour $=\frac{134}{2}=67$, when referred to hydrogen, and 4·63 when referred to air Hence we may conclude that in oxide of triethylphosphine the elements are condensed in the same manner as in the majority of thoroughly investigated organic compounds.

From the facility with which triethylphosphine is converted into the oxide by exposure to the air, even at ordinary temperatures, and the very high boiling-point of the resulting compound, in consequence of which the vapour of the latter can exert but a very slight tension at the common temperature, I am induced to think that the phosphorus-base may be used in many cases for the volumetric estimation of oxygen. When a paper ball soaked in triethylphosphine is passed up in a portion of air confined over mercury, the mercury immediately begins to rise, and continues to do so for about two hours, after which the volume becomes constant, the diminution corresponding very nearly to the proportion of oxygen in the air To obtain very exact results, however, it would be probably necessary in every case to remove the residual vapour of triethylphosphine by means of a ball saturated with sulphuric acid

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* $H_2O = 2$ vols vapour

Oxide of triethylphosphine exhibits in general but a small tendency to unite with other bodies Nevertheless it forms crystalline compounds with iodide and bromide of zinc I have examined more particularly the iodine-compound

Oxide of Triethylphosphine and Iodide of Zinc —On mixing the solutions of the two bodies, the compound separates, either as a crystalline precipitate or in oily drops which soon solidify with crystalline structure It is easily purified by recrystallization from alcohol, when it is deposited in often well-formed monoclinic crystals containing $C_6H_{15}PO, ZnI = (C_2H_5)_3PO, ZnI$

It is remarkable that this compound formed in presence of a large excess of hydriodic and even of hydrochloric acid

Oxide of Triethylphosphine and Dichloride of Platinum.—No precipitate is formed on mixing the aqueous solutions of the two compounds, however concentrated But on adding the anhydrous oxide to a concentrated solution of dichloride of platinum in absolute alcohol, a crystalline platinum-compound is deposited after a few moments. This compound is exceedingly soluble in water, easily soluble in alcohol, insoluble in ether On adding ether to the alcoholic solution, the salt is precipitated, although with difficulty, in the crystalline state The alcoholic solution, when evaporating spontaneously, yields beautiful hexagonal plates of the monoclinic system, frequently of very considerable dimensions. The crystals have the rather complex formula

$$C_{24}H_{60}P_4O_3Pt_2Cl_6 = 3[(C_2H_5)_3PO] + (C_2H_5)_3PCl_2, 2PtCl_2$$

On mixing the concentrated solution of the oxide with trichloride of gold, a deep yellow oil is separated, which crystallizes with difficulty after considerable standing. This compound is exceedingly soluble in water and in alcohol When the aqueous solution is heated, the gold is reduced , the transformation which the oxide of triethylphosphine undergoes in this reaction is not examined.

Chloride of tin forms likewise an oily compound with the oxide · I have not succeeded in crystallizing this compound.

Chloride of mercury is without any action on oxide of triethylphosphine.

Oxychloride of Triethylphosphine —On passing a current of dry hydrochloric acid through a layer of oxide of triethylphosphine which is fused in a U-shaped tube surrounded by boiling water, brilliant crystals are soon deposited. These crystals disappear, however, rapidly, the compound formed in the commencement of the reaction uniting with an excess of hydrochloric acid. The viscous liquid which ultimately remains behind, when heated loses the excess of hydrochloric acid, leaving an exceedingly deliquescent crystalline mass, very soluble in alcohol, insoluble in ether

For analysis, the new compound was washed with absolute ether and dried over sulphuric acid *in vacuo*, either at the common temperature or at 40°. Three chlorine-determinations in specimens of different preparations, which, owing to the extraordinary avidity of this compound for moisture, exhibit greater discrepancies than are generally observed in experiments of this description, lead to the formula $C_{12} H_{30} P_2 O Cl_2 = (C_2 H_5)_3 PO, (C_2 H_5)_3 PCl_2$.
The dichloride of triethylphosphine cannot be formed by the action of hydrochloric acid upon the oxide.

The oxychloride exhibits with other compounds the deportment of the oxide. It furnishes with dichloride of platinum the same platinum-salt which is obtained with the oxide In a similar manner it gives with iodide of zinc the iodide of zinc-compound of the oxide previously described. Only once—under conditions not sharply enough observed at the time, and which I was afterwards unable to reproduce in repeated experiments — a compound of the oxychloride with iodide of zinc was formed. This substance, readily soluble in water and alcohol, crystallized from the latter solvent in beautiful colourless, transparent octahedra of the composition

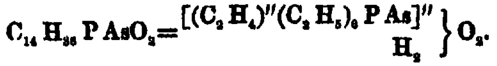
$$C_{12} H_{30} P_2 O Cl_2 Zn_2 I_2 = (C_2 H_5)_3 PO, (C_2 H_5)_3 PCl_2, 2ZnI.$$

XIII. "Researches on the Phosphorus-Bases"—No IX. Phospharsonium Compounds. By A. W. Hofmann, LL.D, F R.S Received July 24, 1860

The facility with which the bromide of bromethyl-triethylphosphonium furnishes, when submitted to the action of ammonia and monamines, the extensive and well-defined group of phosphammonium-compounds, induced me to try whether similar diatomic bases containing phosphorus and arsenic might be formed by the mutual reaction between the bromethylated bromide and *monarsines* There was no necessity for entering into a detailed examination of this class of compounds I have, in fact, been satisfied to establish by a few characteristic numbers the existence of the phospharsonium-group.

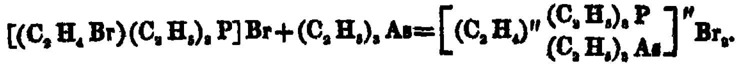
Action of Triethylarsine on Bromide of Bromethyl-triethylphosphonium

On digesting the two substances in sealed tubes at 100°, the usual phenomena are observed, the reaction being complete after the lapse of twenty-four hours The saline mass which is formed yields with oxide of silver in the *cold*, a powerfully alkaline solution, containing the hydrated oxide of ethylene-hexethylphospharsonium,



$$C_{14}H_{36}PAsO_2 = \left.\begin{matrix}[(C_2H_4)''(C_2H_5)_6PAs]'' \\ H_2\end{matrix}\right\}O_2.$$

It is thus obvious that the arsenic-base imitates triethylphosphine in its deportment with the brominated bromide The two substances simply combine to form the dibromide of the phospharsonium,



$$[(C_2H_4Br)(C_2H_5)_3P]Br + (C_2H_5)_3As = \left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P \\ (C_2H_5)_3As\end{matrix}\right]''Br_2.$$

The alkaline solution of the oxide of the phospharsonium exhibits the leading characters of this class of bases, I may therefore refer to the account which I have given of the oxide of diphosphonium. The saline compounds likewise resemble those of the diphosphonium. The dichloride and the di-iodide were obtained in beautiful crystalline needles, exhibiting a marked tendency to form splendidly crystallized double compounds I have prepared the compounds of the dichloride with chloride of tin, bromide of zinc, trichloride of gold, and lastly with dichloride of platinum. The latter compound was analysed in order to fix the composition of the series.

Platinum-salt —The product of the reaction of triethylarsine upon the bromethylated bromide was treated with oxide of silver in the *cold*, and the alkaline solution thus obtained, saturated with hydrochloric acid and precipitated with dichloride of platinum. An exceedingly pale-yellow, apparently amorphous precipitate of diphosphonic appearance was thrown down, almost insoluble in water, but dissolving in boiling concentrated hydrochloric acid The hydrochloric solution deposited, on cooling, beautiful orange-red crystals, resembling those of the diphosphonium-platinum-salt The crystals, according to the measurement of Quintino Sella, belong to the trimetric system The analysis of the platinum-salt led to the formula

$$C_{14}\,H_{34}\,P\,As\,Pt_2\,Cl_6 = \left[(C_2\,H_4)'' \begin{matrix}(C_2\,H_5)_3\,P\\(C_2\,H_5)_3\,As\end{matrix}\right]''Cl_2,\ 2\,Pt\,Cl_2$$

The phospharsonium-compounds, and more especially the hydrated oxide of the series, are far less stable than the corresponding terms of the diphosphonium- and even of the phosphammonium-series. If the product of the action of triethylarsine upon the brominated bromide be *boiled* with oxide of silver instead of being treated in the cold, not a trace of the phospharsonium-compound is obtained. The caustic solution which is formed, when saturated with hydrochloric acid and precipitated with dichloride of platinum, furnishes only the rather soluble octahedral crystals of the oxethylated triethylphosphonium-platinum-salt.* The nature of this transformation is clearly exhibited when a solution of the dioxide of phospharsonium is submitted to ebullition Immediately the clear solution is rendered turbid from separated triethylarsine, which becomes perceptible, moreover, by its powerful odour, the liquid then containing the oxide of the oxethylated triethylphosphonium,

$$\left.\begin{matrix}[(C_2H_4)''(C_2H_5)_6PAs]''\\H_2\end{matrix}\right\}O_2 = (C_2H_5)_3As + \left.\begin{matrix}[(C_2H_4O)(C_2H_5)_3P]\\H\end{matrix}\right\}O.$$

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* See the following Abstract

XIV. "Researches on the Phosphorus-Bases." No X.—Metamorphoses of Bromide of Bromethylated Triethylphosphonium. By A. W. Hofmann, LL.D., F R.S. Received July 24, 1860

Among the several products of transformation into which the bromide of bromethyl-triethylphosphonium is converted when submitted to the action of reagents, the substances formed by its union with bodies similar to ammonia, have hitherto almost exclusively occupied my attention. I have, however, of late examined a variety of other changes of this body, which deserve to be noticed

When heated, the bromide begins to evolve hydrobromic acid at a temperature of about 200°, which continues for a considerable length of time. The product of this reaction is evidently the bromide of vinyl-triethylphosphonium,

$$[(C_2H_4Br)(C_2H_5)_3P]Br = HBr + [(C_2H_3)(C_2H_5)_3P]Br.$$

It is, however, difficult to obtain the substance pure by this process, since the temperature at which the last portion of hydrobromic acid is eliminated closely approximates the degree of heat at which the vinyl-body is entirely destroyed, and since the latter compound may be obtained with the greatest facility by other processes*, I have not followed up any further this direction of the inquiry.

I have already mentioned, in a previous note, the deportment of

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* The hydrated di-oxide of ethylene-hexethyl-diphosphonium, when submitted to distillation, undergoes decomposition, two different phases are to be distinguished in this metamorphosis At about 200° the base begins to disengage the vapour of triethylphosphine, the residuary solution retaining hydrated oxide of vinyl-triethylphosphonium,

$$\left.\begin{matrix}[(C_2H_4)''(C_2H_5)_6P_2]'' \\ H_2\end{matrix}\right\}O_2 = (C_2H_5)_3P + H_2O + \left.\begin{matrix}[(C_2H_3)(C_2H_5)_3P] \\ H\end{matrix}\right\}O,$$

the latter yielding at a higher temperature the oxide of triethylphosphine together with ethylene,

$$\left.\begin{matrix}[(C_2H_3)(C_2H_5)_3P] \\ H\end{matrix}\right\}O = C_2H_4 + (C_2H_5)_3PO.$$

The vinyl-compound is even more readily obtained by the action of silver-salts, such as acetate of silver, at the temperature of 100°, on the bromethylated bromide.

$$(C_2H_4Br)(C_2H_5)_3P]Br + 2\left[\left.\begin{matrix}(C_2H_3O) \\ Ag\end{matrix}\right\}O\right] = 2AgBr + \left.\begin{matrix}(C_2H_3O) \\ [(C_2H_3)(C_2H_5)_3P]\end{matrix}\right\}O + \left.\begin{matrix}(C_2H_3O) \\ H\end{matrix}\right\}O.$$

the bromethylated bromide with oxide of silver, the whole of the bromine is eliminated in the form of bromide of silver, a new base being formed.

According to circumstances, this base may be the vinyl-compound previously mentioned, or another body differing from the latter by containing the elements of one molecule of water in addition. This substance, which is always formed when the reaction takes place in moderately dilute solutions, is the oxide of a phosphonium, with three molecules of ethyl substituted for three equivalents of hydrogen, the fourth equivalent of hydrogen being replaced by an oxygenated radical C_2H_5O, arising from the radical C_2H_4Br by the insertion of HO in the place of Br

$$(C_2H_4Br)(C_2H_5)_3P]Br + 2\left.\begin{matrix}Ag\\H\end{matrix}\right\}O = 2AgBr + \left.\begin{matrix}[(C_2H_4HO)(C_2H_5)_3P]\\H\end{matrix}\right\}O.$$

I have fixed the nature of this compound by the analysis of the iodide, of the platinum-salt and of the gold-salt, and, moreover, by the study of several remarkable transformations which it undergoes when submitted to the action of reagents

It appeared of some interest to ascertain whether the *oxethylated* might be reconverted into the *bromethylated* base The chloride of the former is energetically attacked by pentabromide of phosphorus, oxybromide of phosphorus and hydrobromic acid are abundantly evolved, and the residue of the reaction contains the chloride of bromethylated triethylphosphonium.

$$(C_2H_5O)(C_2H_5)_3P]Cl + PBr_5 = HBr + POBr_3 + [(C_2H_4Br)(C_2H_5)_3P]O.$$

Thus it is seen that the molecular group C_2H_5O, which we assume as hydrogen-replacing in this salt, suffers under the influence of pentabromide of phosphorus, alterations identical with those which it is known to undergo under similar circumstances, when conceived as a constituent of alcohol

If we consider the facility with which the bromethylated triethylphosphonium is converted into the oxethylated compound, by the action of oxide of silver, and the simple re-formation of the first-mentioned body by means of pentabromide of phosphorus, a great variety of new experiments suggest themselves. As yet but little progress has been made in this direction, one of the reactions, however, which I have studied deserves even now to be mentioned.

The salts of bromethylated and oxethylated triethylphosphonium may be regarded as tetrethyl-phosphonium-salts, in which an equivalent of hydrogen in one of the ethyl-molecules is replaced by bromine and by the molecular group HO respectively

Bromide of tetrethylphosphonium	$[(C_2H_4H)(C_2H_5)_3P]Br$,
Bromide of bromethylated triethylphosphonium	$[(C_2H_4Br)(C_2H_5)_3P]Br$,
Bromide of oxethylated triethylphosphonium	$[(C_2H_4HO)(C_2H_5)_3P]Br$,

and the question arose, whether the bromethylated compound might not be converted, simply by reduction, into a salt of tetrethylphosphonium This transformation may, indeed, be effected without difficulty On acidulating the solution of the bromethylated bromide with sulphuric acid and digesting the mixture with granulated zinc, the latent bromine is eliminated as hydrobromic acid, its place being at the same time filled by 1 equiv. of hydrogen,

$$[(C_2H_4Br)(C_2H_5)_3P]Br+2H=HBr+[(C_2H_5)_4P]Br.$$

It was chiefly the facility with which a tetrethyl-phosphonium-compound may be obtained from the brominated bromide, that induced me to designate the hydrogen-replacing molecules C_2H_4Br, and C_2H_5O, which we meet with in the compounds above described, as *bromethyl* and *oxethyl* It remained to be ascertained whether these compounds might actually be formed by means of direct substitution-products of ethyl-compounds It was with the view of deciding this question that I have examined the deportment of triethylphosphine with the monochlorinated chloride and the monobrominated bromide of ethyl.

The former of these substances has been long known, having been investigated by Regnault many years ago, the latter had not been hitherto obtained. I have prepared it by submitting bromide of ethyl to the action of dry bromine under pressure*. It is a heavy aromatic liquid boiling at 110°.

The chlorinated chloride and the brominated bromide of ethyl, although essentially different from their physical properties from dichloride and dibromide of ethylene, with which they are isomeric, nevertheless resemble the ethylene-compounds in their deportment with triethylphosphine.

In both cases the final product of the reaction is a salt of hexethylated ethylene-diphosphonium I have identified these salts with those obtained by means of dichloride and dibromide of ethylene, both by a careful examination of their physical properties, and by the analysis of the characteristic iodide and of the platinum-salt I have not been able to trace in the first of these reactions a salt of chlorethylated triethylphosphonium, but I have established by experiment that in the reaction between triethylphosphine and brominated bromide of ethyl, the formation of bromethyl-triethylphosphonium invariably precedes the production of the diphosphonium-compound

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* In addition to the monobrominated bromide of ethyl, $(C_4H_4Br)Br$, there is also formed in this reaction the dibrominated bromide, $(C_4H_3Br_2)Br$

XV "Researches on the Phosphorus-Bases."—No XI Experiments in the Methyl- and in the Methylene-Series.

By A W Hofmann, LL.D , F R.S Received July 24, 1860.

In former notes I have repeatedly called attention to the transformation of the bromide of bromethylated triethylphosphonium under the influence of bases In continuing the study of these reactions, I was led to the discovery of a very large number of new compounds, the more important ones of which are briefly mentioned in this abstract

Hybrids of Ethylene-diphosphonium.

Action of Trimethylphosphine upon Bromide of Bromethyl-triethylphosphonium.

These two bodies act upon each other with the greatest energy, and moreover exactly in the manner indicated by theory The resulting compound was of course examined only so far as was necessary to establish the character of the reaction

The dibromide of the hybrid diphosphonium is more soluble than the hexethylated compound formerly described, which in other respects it resembles. Oxide of silver eliminates the extremely caustic base

$$C_{11}H_{30}P_2O_2 = \left.\begin{matrix}[(C_2H_4)''(CH_3)_3(C_2H_5)_3P_2]'' \\ H_2\end{matrix}\right\} O_2,$$

which yields with hydrochloric acid and dichloride of platinum a pale-yellow platinum-salt,

$$C_{11}H_{28}P_2Pt_2Cl_6 = \left[(C_2H_4)''\begin{matrix}(CH_3)_3P \\ (C_2H_5)_3P\end{matrix}\right]''Cl_2, 2PtCl_2,$$

separating in scales from boiling water

The salts of the hybrid diphosphonium crystallise like those of the hexethylated diphosphonium, but, so far as they have been examined, are somewhat more soluble. This remark applies especially to the iodide

It seemed worth while to try whether the bromide of brom-ethylated triethylphosphonium was capable of fixing a molecule of phosphoretted hydrogen. It was found, however, that the two bodies do not act upon one another. Phosphoretted hydrogen gas, passed through the alcoholic solution of the bromide, either cold or boiling, did not seem to affect it in any way.

Action of Trimethylphosphine on Dibromide of Ethylene.

This reaction exhibits a repetition of all the phenomena observed in that which takes place between the dibromide and triethylphosphine. The process is completed sooner, if possible, than in the ethyl-series. The lower boiling-point and the overpowering odour of trimethylphosphine render it advisable to mix the materials with considerable quantities of alcohol or ether, and on account of the extreme oxidability of the phosphorus-compound, it is best to operate in vessels filled with carbonic acid and subsequently sealed before the blowpipe. After digestion for a short time at 100°, the mixture of the two liquids solidifies to a hard, dazzling, white, crystalline mass containing the two bromides,

$$C_5 H_{13} P Br_2 = [(C_2 H_4 Br)(C H_3)_3 P] Br,$$

$$C_8 H_{22} P_2 Br_2 = \left[(C_2 H_4)'' \begin{matrix} (C H_3)_3 P \\ (C H_3)_3 P \end{matrix} \right]'' Br_2,$$

one or the other predominating according to the proportions in which the two bodies were allowed to act upon one another.

It was not difficult to establish the nature of these two compounds by numbers.

The solution of the saline mass in absolute alcohol, deposits, on cooling, beautiful prismatic crystals, consisting of the bromide of bromethyl-trimethylphosphonium almost chemically pure, while the diphosphonium-bromide remains in solution. The nature of the monophosphonium-compound was fixed by a bromine determination in the bromide, and by the analysis of a platinum-salt beautifully crystallized in needles containing

$$C_5H_{13}BrPPtCl_3=[(C_2H_4Br)(CH_3)_3P]Cl, PtCl_2.$$

Treatment of this platinum-salt with sulphuretted hydrogen yielded an extremely soluble and deliquescent chloride, which was not analysed, but submitted to the action of oxide of silver, when it furnished the oxide of the corresponding oxethylated compound

$$C_5H_{16}PO_2=\left.\begin{matrix}[(C_2H_5O)(CH_3)_3P]\\ H\end{matrix}\right\}O.$$

The caustic liquid was converted by hydrochloric acid into the easily soluble chloride corresponding to the oxide, and this chloride, when treated with dichloride of platinum, deposited the platinum-salt of the oxethylated trimethylphosphonium in well-formed octahedra extremely soluble in water, containing

$$C_5H_{14}POPtCl_3=[(C_2H_5O)(CH_3)_3P]Cl, PtCl_2$$

Salts of Hexmethylated Ethylene-diphosphonium

Dibromide —The preparation of this salt has already been mentioned   It is extremely soluble in water, and even in absolute alcohol, insoluble in ether   *In vacuo* over sulphuric acid it solidifies into a mass of acicular crystals, which are exceedingly deliquescent

The dibromide, treated with oxide of silver, yields the corresponding dioxide,

$$C_8H_{24}P_2O_2=\left.\begin{matrix}[(C_2H_4)''(CH_3)_6P_2]''\\ H_2\end{matrix}\right\}O_2,$$

which forms with acids a series of salts resembling the corresponding ethyl-compounds. Of these I have prepared only the

Di-iodide, which crystallizes in difficultly soluble needles of the composition

$$C_8H_{22}P_2I_2=\left[(C_2H_4)''\begin{matrix}(CH_3)_3P\\(CH_3)_3P\end{matrix}\right]''I_2,$$

surpassing in beauty the corresponding ethyl-compound, and the—

Platinum-salt.—This is an apparently amorphous precipitate, which is nearly insoluble in water, dissolves with extreme slowness in boiling hydrochloric acid, and separates therefrom on cooling in golden-yellow laminæ, very much like those of the platinum-salt of the hybrid ethylene-trimethyl-triethyl-diphosphonium. It consists of—

$$C_8H_{22}P_2Pt_2Cl_6=\left[(C_2H_4)''\begin{matrix}(CH_3)_3P\\(CH_3)_3P\end{matrix}\right]''Cl_2, 2PtCl_2.$$

METHYLENE GROUP.

Action of Triethylphosphine on Di-iodide of Methylene

Triethylphosphine and di-iodide of methylene act so powerfully on one another, that it is necessary to moderate the reaction by the presence of a considerable quantity of ether The reaction is very soon completed, even when the mixture is largely diluted, especially if it be heated to 100° in sealed tubes The saline residue left after the evaporation of the ether is immediately seen to be a mixture of several compounds, one of which—a *sparingly* soluble iodide crystallizing in long needles—at once arrests attention

From analogy we might expect to find in the saline mixture the compounds

$$[(CH_2I)(C_2H_5)_3P]I,$$

or

$$[(CH_2)''(C_2H_5)_6P_2]''I_2.$$

Experiment has, however, established the presence of the first only

The difficultly soluble crystals just mentioned are easily purified, being readily soluble in water, sparingly in alcohol, insoluble in ether Their solution in boiling alcohol yields splendid needles frequently an inch long, and possessing extraordinary lustre. Analysis prove this beautiful salt to be the first of the above-mentioned compounds.

The new iodide behaves with nitrate of silver like the bromide of bromethylated triethylphosphonium; *half the iodine is eliminated* in the form of iodide of silver It differs, however, from the bromide in its deportment with oxide of silver which, after removal of the accessible iodine, leaves the latent iodine untouched, even after protracted ebullition. A powerfully alkaline solution is thus obtained containing the base

$$C_7H_{18}IPO = \left.\begin{matrix}[(CH_2I)(C_2H_5)_3P] \\ H\end{matrix}\right\}O.$$

The crystals of the iodide were transformed into the chloride by means of chloride of silver, and the solution was precipitated by dichloride of platinum. The precipitate is very sparingly soluble in cold water, but may be recrystallized from a considerable quantity of boiling water. As the liquid cools, splendid needle-shaped crystals are deposited containing

$$C_7H_{17}IPPtCl_3 = [(CH_2I)(C_2H_5)_3P]Cl, PtCl_2.$$

The sparingly soluble iodide is present in proportionally small quantity only among the products of the action of di-iodide of methylene on triethylphosphine. I have in vain endeavoured to detect among these products anything of the nature of a diphosphonium-compound. On treating the mother-liquor of the sparingly soluble iodide with chloride of silver, and the dilute filtered solution with dichloride of platinum, a few needles of the iodated platinum-salt are still deposited, but after considerable evaporation the solution yields crystals, all of which exhibit an octahedral habitus. I was equally unsuccessful in a particular experiment, in which I subjected di-iodide of methylene to the action of a large excess of triethylphosphine, and a similar report must be made of the attempt to produce the desired body by treating the ready prepared iodide with triethylphosphine, according to the equation

$$[(CH_2I)(C_2H_5)_3P]I + (C_2H_5)_3P = [(CH_2)''(C_2H_5)_6P_2]''I_2.$$

The examination of the mother-liquor of the sparingly soluble iodide is a difficult and thankless proceeding, nevertheless, by a sufficient number of iodine- and platinum-determinations, it may be shown to be a mixture of four different compounds The mother-liquor is thus found to contain, together with the hydriodate of the phosphorus-base, two crystallizable iodides differing in solubility, and to be separated from one another only by a great number of crystallizations.

The more soluble salt is the iodide of oxymethylated triethylphosphonium, corresponding to the iodomethylated compound; the less soluble salt is the iodide of methyl-triethylphosphonium. The last mother-liquors contain considerable quantities of oxide of triethylphosphine.

Iodide of Oxymethyl-triethylphosphonium.

This salt is extremely soluble both in water and in alcohol, even in absolute alcohol, and crystallizes only after the alcohol has been completely evaporated The crystals, resembling the frosty efflorescences on a window-pane, contain

$$C_7H_{18}OPI = [(CH_3O)(C_2H_5)_3P]I$$

The iodide, treated with oxide of silver, is converted into the corresponding caustic oxide, which, when mixed with hydrochloric acid and dichloride of platinum, yields a rather easily soluble platinum-salt of an octahedral habitus.

Iodide of Methyl-triethylphosphonium.

The nature of the less soluble iodide was determined by an iodine-determination, and by the analysis of the platinum-salt. The iodide dissolves in water and in alcohol, but is insoluble in ether. By adding ether to the alcoholic solution, tolerable crystals are obtained. This compound is most conveniently purified by precipitating the alcoholic mother-liquor, after freeing it by crystallization as far as possible from the iodomethylated iodide, with a quantity of ether insufficient to precipitate the whole, so that the greater part of the iodides may remain in solution.

The iodide thus prepared contains

$$C_7H_{18}PI = [(CH_3)(C_2H_5)_3P]I.$$

For further verification of this formula the crystals were deiodized with silver-oxide, and the caustic liquid thus obtained was saturated with hydrochloric acid and precipitated by dichloride of platinum. The platinum-salt, which crystallizes in beautiful octahedra, was found to contain

$$C_7H_{18}PPtCl_3 = [(CH_3)(C_2H_5)_3P]Cl, PtCl_2$$

The two iodides are accompanied by a considerable quantity of oxide of triethylphosphine, which immediately separates in oily drops on treating the last mother-liquor with potash. Its presence was likewise unmistakeably recognized by the preparation of the platinum-salt. If the last mother-liquor of the iodine-compounds be deiodized and mixed with hydrochloric acid and dichloride of platinum, a quantity of octahedral salts separates in the first place, which are removed by sufficient concentration, the remaining liquid, when mixed with alcohol and ether, yields a crystalline precipitate, which separates from alcohol by spontaneous evaporation in the beautiful large hexagonal tables consisting of the platinum-salt of the oxychloride of triethylphosphine, which has been more fully described in one of the previous notes on these researches.

The formation of the four compounds contained in the mother-liquor of the sparingly soluble iodide is illustrated by the following equations:—

$$2[(C_2H_5)_3P] + CH_2I_2 + H_2O = [(C_2H_5)_3HP]I + [(CH_2O)(C_2H_5)_3P]I$$

$$[(3C_2H_5)_3P] + CH_2I_2 + H_2O = [(C_2H_5)_3HP]I + [(CH_3)(C_2H_5)_3P]I + (C_2H_5)_3PO.$$

XVI. "Researches on the Phosphorus-Bases."—No. XII. Relations between the Monoatomic and the Polyatomic Bases By A. W. HOFMANN, LL.D., F.R.S. Received August 17, 1860

In recording my experiments on the derivatives of triethylphosphine, I have had more than one opportunity of alluding to the energy and precision which characterize the reactions of this compound. The usefulness of triethylphosphine as an agent of research has more particularly manifested itself in the study of the polyatomic bases, the examination of which, in continuation of former inquiries, was naturally suggested by the beautiful researches on the polyatomic alcohols published during the last few years. In the commencement these studies were almost exclusively performed with reference to derivatives of ammonia, but the results obtained in the examination of triethylphosphine have, in a great measure, changed the track originally pursued, and of late I have generally preferred to solve the problems which I had proposed to myself, by the aid of the phosphorus-bases.

The light which the study of these compounds throws upon the nature of the polyatomic bases generally, will be fully appreciated by a retrospective glance at the deportment of triethylphosphine under the influence of dibromide of ethylene, and a comparison of the products formed in this reaction with the results suggested by theory.

A simple consideration shows that the action of diatomic bromides upon bases must give rise to the formation of several classes of compounds. Let us examine by way of illustration the products which may be expected to be formed in the reaction between ammonia and dibromide of ethylene.

The diatomic bromide being capable of fixing two molecules of ammonia, we have in the first place four diatomic bromides of the formulæ

$$[(C_2H_4)''\ H_6N_2]''\ Br_2$$
$$[(C_2H_4)_2''\ H_4N_2]''\ Br_2$$
$$[(C_2H_4)_3''\ H_2N_2]''\ Br_2$$
$$[(C_2H_4)_4''\ N_2]''\ Br_2.$$

These are, however, by no means the only salts which, in accordance with our present conception of diatomic compounds, may be formed in this reaction Taking into consideration the general deportment of dibromide of ethylene, there could be no doubt that, under certain conditions, this body would act with ammonia as a monoatomic compound, giving rise to another series of bodies, in which the hydrogen would be more or less replaced by the monoatomic molecule $C_2 H_4 Br$, viz.

$$[(C_2 H_4 Br) H_3 N] Br$$
$$[(C_2 H_4 Br)_2 H_2 N] Br$$
$$[(C_2 H_4 Br)_3 H N] Br$$
$$[(C_2 H_4 Br)_4 N] Br$$

Further, if the reaction took place in the presence of water, it was to be expected that the latent bromine of these salts, wholly or partially eliminated in the form of hydrobromic acid, would be replaced by the molecular residue of water, and thus, independently of any mixed compounds containing simultaneously bromine and oxygen, a series of salts might be looked for, in which a molecule $C_2 H_4 (HO) = C_2 H_5 O$ would enter monoatomically

$$[(C_2 H_5 O) H_3 N] Br$$
$$[(C_2 H_5 O)_2 H_2 N] Br$$
$$[(C_2 H_5 O)_3 H N] Br$$
$$[(C_2 H_5 O)_4 N] Br.$$

Lastly, remembering the tendency exhibited by ethylene-compounds to resolve themselves in the presence of alkalies into vinyl-products, it appeared not improbable that a fourth series of bodies would likewise be formed,

$$[(C_2 H_3) H_3 N] Br$$
$$[(C_2 H_3)_2 H_2 N] Br$$
$$[(C_2 H_3)_3 H N] Br$$
$$[(C_2 H_3)_4 N] Br$$

In the experiments on the action of dibromide of ethylene upon ammonia, which I have already partly published, and which, in a more connected form, I hope soon to lay before the Royal Society, I have not, indeed, met with the whole of these compounds, but in the place of the deficient members of the groups new products have made their appearance, whose formation in the present state of our knowledge could scarcely have been predicted, and thus the problem of disentangling the difficulties of this reaction becomes a task of very considerable difficulty Nor did the action of dibromide of ethylene upon ethylamine, diethylamine, and triethylamine, which I subsequently studied, afford a sufficiently simple expression of the transformations suggested by theory The difficulties disappeared at once when the experiment was repeated in the phosphorus-series In the reaction with dibromide of ethylene, the sharply-defined characters of triethylphosphine exhibited themselves with welcome distinctness, and in consequence more especially of the absence of unreplaced hydrogen—whereby the formation of a large number of compounds of subordinate theoretical interest was excluded—the general character of the reaction, the recognition of which was the object of the inquiry, became at once perceptible

I have shown that the action of dibromide of ethylene upon triethylphosphine gives rise to the formation of four different compounds, viz

$$[(C_2H_4)'' \quad (C_2H_5)_6P_2]''Br_2$$
$$[(C_2H_4Br)(C_2H_5)_3P]\ Br$$
$$[(C_2H_5O)\ (C_2H_5)_3P]\ Br$$
$$[(C_2H_3) \quad (C_2H_5)_3P]\ Br,$$

each of which represents one of the *four* groups of compounds, which under favourable circumstances may arise from the mutual reaction between ammonia and dibromide of ethylene, the production of a greater number of terms being impossible on account of the ternary substitution of triethylphosphine

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Whilst going on with the researches on the phosphorus-bases which I have taken the liberty of submitting to the Royal Society, in notes sketched as I advanced, I have not altogether lost sight of the experiments in the nitrogen-series, which had originally suggested these inquiries Numerous nitrogenated bases, both monoatomic and diatomic, with which I have become acquainted during this investigation, must be reserved for a future communication I may here only remark, that these substances, although differing in several points, nevertheless imitate in their general deportment so closely the corresponding terms of the phosphorus-series, that the picture which I have endeavoured to delineate of the phosphorus-compounds, illustrates in a great measure the history of the nitrogen-bodies.

In conclusion, a few words about the further development of which the experiments on the polyatomic bases appear to be capable, and about the direction in which I propose to pursue the track which they have opened.

Conceived in its simplest form, the transition from the series of monoatomic to that of diatomic bases, may be referred to the introduction of a monochlorinated or a monobrominated alcohol-radical into the type ammonia, the chlorine and bromine thus inserted furnishing the point of attack for a second molecule of ammonia.

If in bromide of ethylammonium—to pass from the phosphorus-series to the more generally interesting nitrogen-series—we replace 1 equiv of the hydrogen in ethyl by bromine, we arrive at bromide of bromethylammonium, which fixing a second equivalent of ammonia, is converted into the dibromide of ethylene-diammonium, the latent bromine becoming accessible to silver-salts

$$[(C_2\,H_4\,Br)\,H_3\,N]\,Br + H_3\,N = [(C_2\,H_4)''\,H_6\,N_2]''\,Br_2$$

The further elaboration of this reaction indicates two different methods for the construction of the polyatomic bases of a higher order. In the first place, the number of ammonia-molecules, to be *incorporated in the new system*, may be increased by the *gradually* advancing bromination of the radical By the further bromination of ethyl in bromide of bromethylammonium and the action of ammonia on the bodies thus produced, the following salts may be generated :-

$$[(C_2\,H_3\,Br_2)\,H_3\,N]\,Br + 2H_3\,N = [(C_2\,H_3)'''\;\;H_9\;N_3]'''\;\;Br_3$$
$$[(C_2\,H_2\,Br_3)\,H_3\,N]\,Br + 3H_3\,N = [(C_2\,H_2)''''\;H_{12}\,N_4]''''\;Br_4$$
$$[(C_2\,H\;\;Br_4)\,H_3\,N]\,Br + 4H_3\,N = [(C_2\,H\;)'''''\;H_{15}\,N_5]'''''\;Br_5$$
$$[(C_2\qquad Br_5)\,H_3\,N]\,Br + 5H_3\,N = [(C_2\qquad)''''''\,H_{18}\,N_6]''''''\,Br_6.$$

Again, the fixation of the ammonia-molecules may be attempted, not by the *progressive bromination of the ethyl*, but by the *accumulation* of monobrominated ethyl-molecules in the ammonium-nucleus. The bromide of di-bromethylammonium, when submitted to the action of ammonia, would thus yield the tribromide of a triammonium, the bromide of tri-bromethylammonium, the tetrabromide of a tetrammonium, and lastly, the bromide of tetrabromethylammonium, the pentabromide of a pentammonium.

$$[(C_2H_4Br)_2H_2N]Br+2H_3N=[(C_2H_4)_2''H_8N_3]'''Br_3$$
$$[(C_2H_4Br)_3H\ N]Br+3H_3N=[(C_2H_4)_3''H_{10}N_4]''''Br_4$$
$$[(C_2H_4Br)_4\ N]Br+4H_3N=[(C_2H_4)_4''H_{12}N_5]'''''Br_5.$$

As yet the bromination of the alcohol-bases presents some difficulty, appropriately selected reactions, however, will doubtless furnish the several brominated bases They may probably be obtained by indirect processes, similar to those by which years ago I succeeded in preparing the chlorinated and brominated derivatives of phenylamine, or these bodies may be generated by the action of pentachloride or pentabromide of phosphorus upon the oxethylated bases, a process, which, to judge from the few experiments recorded in one of the preceding sketches, promises a rich harvest of results

I have but a faint hope that I may be able to trace these new paths in the numerous directions which open in a variety at once tempting and perplexing Inexorable experiment follows but slowly the flight of light-winged theory The commencement is nevertheless made, and even now the triammonium- and tetrammonium-compounds begin to unfold themselves in unexpected variety. One of the most remarkable compounds belonging to the triammonium-group is *diethylene triamine,*

$$C_4H_{13}N_3=\left.\begin{matrix}(C_2H_4)''_2\\ H_5\end{matrix}\right\}N_3$$

This base, *the first triacid triammonia,* forms splendid salts of the formula

$$[(C_2H_4)''_2H_8N_3]'''Cl_3,$$

which will be the subject of a special communication

November 15, 1860.

Major-General SABINE, R.A., Treasurer and Vice-President, in the Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting for the election of Council and Officers

David Forbes, Esq , was admitted into the Society

The following communication was read —

I "On the Laws of the Phenomena of the larger Disturbances of the Magnetic Declination in the Kew Observatory with notices of the progress of our knowledge regarding the Magnetic Storms" By Major-General EDWARD SABINE, R.A., Treas. and V P Received November 15, 1860.

The laws manifested by the mean effects of the larger magnetic disturbances (regarded commonly as effects of magnetic storms) have been investigated at several stations on the globe, being chiefly those of the British Colonial Observatories, but hitherto there has been no *similar examination of the phenomena in the British Islands* themselves. The object of the present paper is to supply this deficiency, as far as one element, namely the declination, is concerned, by a first approximation derived from the photographs in the years 1858 and 1859, of the self-recording declinometer of the observatory of the British Association at Kew, leaving it to the photographs of subsequent years to confirm, rectify, or render more precise the results now obtained by a first approximation The method of investigation is simple, and may be briefly described as follows :—

The photographs furnish a continuous record of the variations which take place in the direction of the declination-magnet, and admit of exact measurement in the two relations of time, and of the amount of departure from a zero line From this automatic record, the direction of the magnet is measured at twenty-four equal intervals of time in every solar day, which thus become the equivalents of the "hourly observations" of the magnetometers in use at the Colonial Observatories. These measures, or hourly directions of the magnet, are entered in monthly tables, having the days of the month in successive horizontal lines, and the hours of the day in vertical columns The "means" of the entries in each vertical column indicate the mean direction of the magnet at the different hours of the month to which the table belongs, and have received the name of "First Normals" On inspecting any such monthly table, it is at once seen that a considerable portion of the entries in the several columns differ considerably from their respective means or first normals, and must be regarded as "disturbed observations" The laws of their relative frequency, and amount of disturbance, in different years, months and hours, are then sought out, by separating for that purpose a sufficient body of the most disturbed observations, computing the amount of departure in each case from the normal of the same month and hour, and arranging the amounts in annual, monthly, and hourly tables In making these computations, the first normals require to be themselves corrected, by the omission in each vertical column of the entries noted as disturbed, and by taking fresh means, representing the normals of each month and hour after this omission, and therefore uninfluenced by the larger disturbances These new means have received the name of "Final Normals," and may be defined as being the mean directions of the magnet in every month and every hour, after the omission from the record of every entry which differed from the mean a certain amount either in excess or in defect.

In this process there is nothing indefinite, and nothing arbitrary save the assignment of the particular amount of difference from the normal which shall be held to constitute the measure of a large disturbance, and which, for distinction sake, we may call "*the separating value.*" It must be an amount which will separate a sufficient body of disturbed observations to permit their laws to be satisfactorily ascertained, but in other respects its precise value is of minor significancy, and the limits within which a selection may be made, without materially affecting the results, are usually by no means narrow, for it has been found experimentally on several occasions, that the *Ratios* by which the periodical variations of disturbance in different years, months and hours are characterized and expressed, do not undergo any material change by even considerable differences in the amount of the separating value The separating value must necessarily be larger at some stations than at others, because the absolute magnitude of the disturbance-variation itself is very different in different parts of the globe, as well as its comparative magnitude in relation to the more regular solar-diurnal variation, but it must be a *constant* quantity throughout at one and the same station, or it will not truly show the relative proportion of disturbance in different years and different months

The strength of the Kew establishment being insufficient for the complete work of a magnetic observatory, the tabulation of the hourly directions from the photographic records has been performed by the non-commissioned officers of the Royal Artillery, employed under my direction at Woolwich, where this work has been superintended by Mr John Magrath, the principal clerk, as have been also the several reductions and calculations, which have been made on the same plan as those of the Colonial Observatories.

In the scale on which the changes of direction of the declination-magnet are recorded in the Kew photographs, one inch of space is equivalent to 22′ 04 of arc On a general view and consideration of the photographs during 1858 and 1859, 0 15 inch, or 3′ 31 of arc appeared to be a suitable amount for the separating value to be adopted at that station, consequently every tabulated value which differed 3′ 31 or more, either in excess or defect from the final normal of the same month and hour, has been regarded as one of the larger disturbances, and separated accordingly The number of disturbed observations in the two years was 2424 (viz 1211 in 1858, and 1213 in 1859), being between one-seventh and one-eighth of the whole body of hourly directions tabulated from the photographs, of which the number was 17,319. The aggregate value of disturbance in the 2424 observations, was 14,901 minutes of arc, of which 7207 minutes were deflections of the north end of the magnet to the west, and 7694 to the east, the easterly deflections thus having a slight preponderance The number of the disturbed observations, as well as their aggregate values, approximated very closely in each of the two years, 1859 being very slightly in excess The *decennial period* of the magnetic storms, indicated by the observations at the British Colonial Observatories between 1840 and 1850, had led to the anticipation that the next epoch of maximum of the cycle might take place in the years 1858–1859 The nearly equal proportions in which the numbers and aggregate values of the larger disturbances took place in 1858 and 1859 are so far in accordance with this view. Should the records of the succeeding years at Kew, made with the same instruments, and examined by the same method, show decreasing disturbance in 1860 and 1861, the precise epoch of the maximum indicated by the records of the Kew declinometer will be "the end of 1858 or commencement of 1859"

In Table I are shown the aggregate values of disturbance in the two years, arranged under the several hours of solar time in which they occurred They are also divided into the two categories of westerly and easterly deflections, since the experience gained at other stations has now fully established that the westerly and easterly disturbance-deflections are characterized in all parts of the globe by distinct and dissimilar laws The Ratios are also shown which the aggregate values at the different hours, both of the westerly and the easterly deflections, bear to their respective mean values,—or, in other words, to the sums respectively of the westerly and easterly deflections at all the hours, divided by 24, and taken as the respective units.

TABLE I —Showing the aggregate values of the larger disturbances of the Declination at the different hours of solar time in 1858 and 1859, derived from the Kew Photographs, with the Ratios of disturbance at the several hours to the mean hourly value taken as the Unit.

Mean astronomical hours	Westerly deflections		Easterly deflections		Mean civil hours
	Aggregate values (Minutes of arc)	Ratios	Aggregate values (Minutes of arc)	Ratios	
18	553·9	1·85	118·9	0·37	6 A.M.
19	549·3	1·83	120·9	0·38	7 A.M.
20	442·9	1·48	115·2	0·36	8 A.M.
21	370·1	1·23	121·2	0·38	9 A.M.
22	376·9	1·26	104·6	0·33	10 A.M.
23	361·8	1·21	125·8	0·39	11 A.M.
0	413·7	1·38	173·0	0·54	Noon.
1	431·1	1·44	153·3	0·48	1 P.M.
2	459·8	1·53	173·0	0·54	2 P.M.
3	513·0	1·71	108·4	0·34	3 P.M.
4	403·9	1·35	141·0	0·44	4 P.M.
5	343·8	1·15	164·8	0·51	5 P.M.
6	282·5	0·94	291·1	0·91	6 P.M.
7	110·7	0·37	381·8	1·19	7 P.M.
8	65·6	0·22	499·0	1·56	8 P.M.
9	88·2	0·29	572·9	1·79	9 P.M.
10	59·0	0·20	724·3	2·25	10 P.M.
11	35·7	0·12	767·8	2·38	11 P.M.
12	146·7	0·49	709·5	2·21	Midnight.
13	141·8	0·47	634·8	1·98	1 A.M.
14	146·7	0·49	577·2	1·80	2 A.M.
15	151·5	0·51	464·8	1·45	3 A.M.
16	289·5	0·97	305·8	0·95	4 A.M.
17	458·9	1·53	144·9	0·45	5 A.M.
Mean hourly value 299·9 = 1·00			Mean hourly value 320·6 = 1·00		

The westerly and easterly deflections in the British Islands, as represented by the automatic records at Kew, are obviously governed, as in all other parts of the globe where the phenomena have been analysed, by distinct laws. The westerly deflections have their chief prevalence from 5 A.M to 5 P M, or during the hours of the *day*, the easterly deflections, on the other hand, prevail chiefly during the hours of the *night*, the ratios being above unity from 7 P M to 3 A M, and below unity at all other hours The easterly have one decided maximum, viz at 11 P M., towards which they steadily and continuously progress from 5 P M, and from which they as steadily and continuously recede until 5 A M. the following morning The westerly deflections appear to have two epochs of maximum, one from 6 to 7 A M, the other about 3 P M, progressing regularly towards the first named from 3 A.M., and receding from it to 9 A M; at 9, 10, and 11 A M the ratios remain almost sensibly the same, but towards noon they begin to increase afresh, and continue to do so progressively to the second maximum at 3 P M, from which hour they progressively decrease to 7 P M Those ratios which are less than unity, viz those of the westerly deflections from 6 P.M to 4 A M, and of the easterly from 4 A M to 6 P.M, do not in either case exhibit the same decided tendency to one or two well-marked minima, as the ratios which are above unity do in both cases towards their maxima. It is possible, however, that this may in some degree be explained by the following consideration —

The aggregate values of the disturbances prevailing at the different hours, as stated in the Table, are those which have prevailed, not only over the forces which would retain the magnet in its *mean* position, but also over any disturbing influences in an opposite direction, which may be conceived to have existed contemporaneously, and we cannot but suppose that as both westerly and easterly disturbances do record themselves as prevailing at the same hours on different days, that these opposite influences may sometimes *coexist*, neutralizing each other and not appearing in the record. We may reasonably suppose that the degree in which the aggregate values in the Table, both westerly and easterly, may be diminished thereby at the different hours, may be in some measure indicated by the disparity, or the reverse, in the amount of the aggregate values of disturbance in the opposite directions at those hours. Thus we may suppose that at a particular hour, 11 P.M. for example, when the amount of westerly deflections is very small, and of easterly very great, the diminution of the aggregate values of either by mutual counterbalance may be extremely small, while of equal absolute amount in both Now a very small amount deducted from the large aggregate easterly value will scarcely have any effect whatsoever on the ratio at that hour to its unit or mean hourly value; whereas the same small amount deducted from the far less aggregate westerly value at the same hour would have a far more sensible effect upon its ratio. Assuming, therefore, the probability that westerly and easterly disturbing influences do sometimes coexist and neutralize each other in the record, and that we may in some degree judge of the respective amounts of the conflicting influences at the several hours by the means above stated, we should be prepared to expect that the ratios which are below unity do not represent the actual variations of the disturbing influences at those hours quite so purely as do the ratios which are above unity, and that they are liable to be affected, though in a very subordinate degree, by the abstraction of the neutralized portion, when the aggregate values which they represent are very small.

Without, however, resting undue weight upon this suggestion, we may safely say that the hours, when the ratios are below unity, are hours of comparative tranquillity, and that their variations from hour to hour are of a far less marked character than during the hours when the ratios exceed unity Thus viewed, the character of the disturbance-diurnal variations may be conceived to have some analogy with that of the phenomena of the regular solar-diurnal variation We may imagine the disturbance-variation (either the westerly or the easterly, it is indifferent which is taken),—divided as it is into two portions, by the ratios being in the one case above, and in the other below unity,—to correspond in one of its divisions to the hours when the sun is above the horizon, in the part of the hemisphere where the disturbance may be imagined to originate, whilst the other division, or that in which the ratios are below unity, and manifest hours of comparative tranquillity, may be viewed as the hours of night at the same locality. The solar hours at a station of observation which are characterized by disturbance ratios above unity, will in such case correspond in absolute time with the hours of the *day* at the supposed originating locality, modified (it may be) by a more or less rapid transmission of the disturbance It will be understood, that in this hypothetical suggestion, the purpose in view is to aid the imagination, if it may be so, in apprehending the *ensemble* of the phenomena as far as they are yet known to us, rather than to advance a theoretical explanation, when we have not yet sufficient facts before us by which it may be judged, it may be remarked, however, that the conception of a double locality of origination of the disturbances (easterly and westerly) in the one hemisphere will present no especial difficulty to those who are conversant with the general facts of terrestrial magnetism.

If our attention be limited to the consideration of the facts observed at a single station, unaccompanied by a view of corresponding phenomena elsewhere, we might be in danger of regarding some of the features, particularly perhaps those which are not the most prominent, as having an accidental rather than a systematic origin; and we might thus lose a portion of the instruction which they may otherwise convey. On this account it has appeared desirable to exhibit the phenomena as observed at a second station, in comparison with those at Kew, and I have selected for this purpose the results of a similar investigation to the present at Hobarton in Tasmania, not only because the facts have been remarkably well determined there, but also because, though it is a very distant station, differing widely in geographical latitude and longitude, and situated indeed in a different hemisphere, there is a striking resemblance in the laws of the magnetic storms experienced at both. This resemblance, which is not only general, but extends to very minute particulars, is such that it seems impossible to resist the impression that the accordance cannot be accidental, and that the methods of observation and of analysis which have been pursued, have proved themselves well adapted to open to us the knowledge of the existence of systematic laws, pervading and regulating the action of the forces which are in daily operation around us, and are *at least* co-extensive with the limits of our globe, and thus to lead us ultimately to the correct theory of these forces. I have placed therefore beside each other in the next Table the Ratios of Disturbance at the different hours of local solar time at each of the two stations, separating them as before into westerly and easterly deflections, and placing the westerly deflections at Kew in immediate juxtaposition with the easterly at Hobarton, and *vice versâ*, as that obviously constitutes the just comparison. The Hobarton Ratios exhibit the relative prevalence of disturbance at the several hours, derived from hourly observations continued for seven years and nine months, viz from January 1, 1841 to September 30, 1848; a series unparalleled in duration at any other of the Colonial Observatories, and which has borne admirably, as I shall hope to have a future opportunity of explaining to the Society, an unquestionable test of its substantial accuracy and fidelity The number of recorded hourly observations was 56,202, of which 7638 differed from their respective normals of the same month and hour by an amount equalling or exceeding 2′ 13 of arc, and constituted the body of separated observations from which the aggregate values of disturbance at the different hours and their ratios have been obtained The proportion of disturbed observations thus separated, to the whole body of observations, is about 1 in 7·35, differing very little from the proportion already noticed as obtained at Kew by a separating value of 3′ 3. The disturbing effects due to magnetic storms are therefore somewhat greater at Kew than at Hobarton, though some portion of the difference may be ascribed to the circumstance, that the terrestrial horizontal force, antagonistic to the disturbing *forces* and *tending to retain* the *magnet in its mean* position, is less at Kew than at Hobarton, in the proportion, approximately, of 3 7 to 4 5

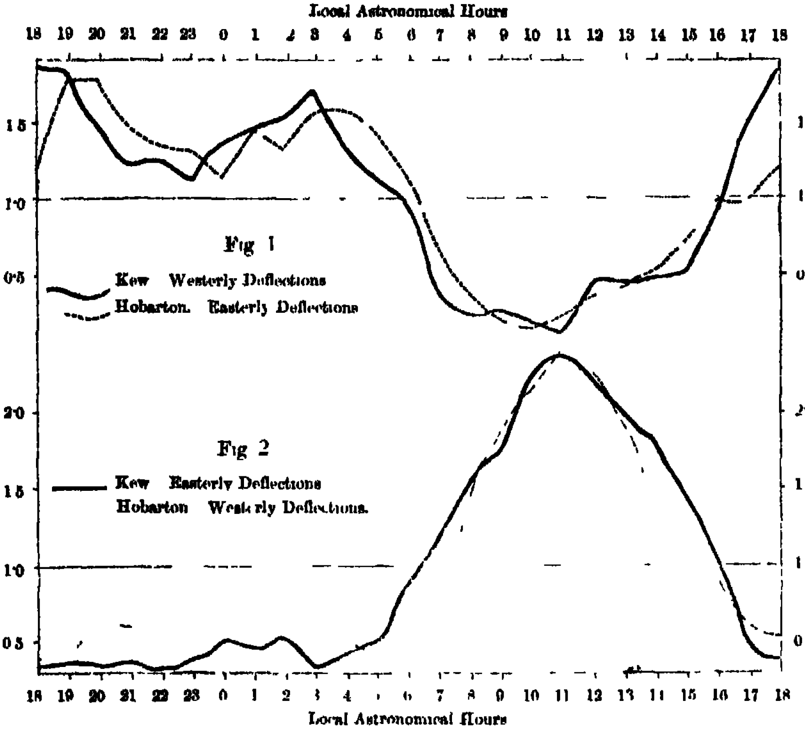
Table II.—Showing the comparison of the Ratios of the larger Disturbances of the Declination at the different hours of local solar time at Kew and Hobarton.

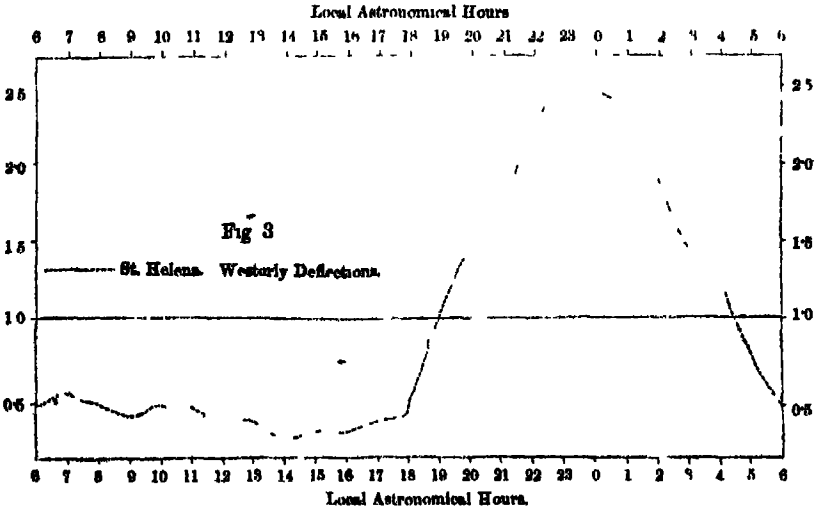
Local astronomical hours	Kew Westerly deflection	Hobarton Easterly deflection	Kew Easterly deflection	Hobarton Westerly deflection	Local civil hours
18	1·85	1·18	0·47	0·42	6 A.M.
19	1·83	1·75	0·38	0·44	7 A.M.
20	1·48	1·76	0·36	0·62	8 A.M.
21	1·23	1·47	0·38	0·60	9 A.M.
22	1·26	1·38	0·33	0·54	10 A.M.
23	1·21	1·31	0·39	0·53	11 A.M.
0	1·38	1·17	0·54	0·67	Noon
1	1·44	1·44	0·48	0·56	1 P.M.
2	1·53	1·31	0·54	0·68	2 P.M.
3	1·71	1·56	0·34	0·60	3 P.M.
4	1·35	1·58	0·44	0·50	4 P.M.
5	1·15	1·41	0·51	0·42	5 P.M.
6	0·94	1·10	0·91	0·68	6 P.M.
7	0·37	0·62	1·19	0·90	7 P.M.
8	0·22	0·37	1·56	1·50	8 P.M.
9	0·29	0·22	1·79	1·87	9 P.M.
10	0·20	0·17	2·25	2·20	10 P.M.
11	0·12	0·22	2·38	2·49	11 P.M.
12	0·49	0·38	2·21	2·15	Mid
13	0·47	0·41	1·98	1·74	1 A.M.
14	0·49	0·53	1·80	1·35	2 A.M.
15	0·51	0·71	1·45	1·25	3 A.M.
16	0·97	1·01	0·95	0·85	4 A.M.
17	1·53	0·96	0·45	0·48	5 A.M.

For the convenience of those who prefer graphical illustration, I have represented on an accompanying woodcut the results to which I have referred. The curves drawn in unbroken black lines, in figures 1 and 2, show the phenomena at Kew; those in dotted lines in the same figures, the phenomena at Hobarton. Fig. 1 presents westerly disturbances at Kew, and easterly at Hobarton in comparison with each other; they are obviously allied phenomena. Fig. 2 presents easterly disturbances at Kew and westerly at Hobarton; these are also, obviously, allied phenomena, but are as obviously governed by distinct laws from those in fig. 1.

Had the phenomena at Kew and Hobarton been the only ones known to us, we might have inferred that we had obtained the characteristic forms of the diurnal variations due to the action of two distinct and independent forces; and we might have expected with some degree of confidence to have found curves of corresponding

Mean Diurnal Disturbance Variation of the Magnetic Declination.
Figs 1 *and* 2, *Kew and Hobarton* *Fig* 3, *St Helena*





form by a similar analysis elsewhere ,—and so far experience has been in accord with expectation But, as the *forms* of these two pair of curves are not only respectively similar, but as they also correspond in the *hours* at which their chief characteristic features occur, we might also have formed an inference which would have proved erroneous, viz. that the hours as well as the forms would be the same at other stations Now this is so far from being in accordance with the facts which we already possess, that whilst the forms present generally a marked resemblance, the hours at different stations exhibit every variety To exemplify this I have given in a third figure the curve of the westerly disturbance diurnal variation at St Helena, of which the form is manifestly the same as that of the two curves in fig 2, whilst the hours of its most marked features exhibit a difference of nearly 12 hours of local time from those in fig 2

It may not be unsuitable on the present occasion to take a brief retrospective view of the progress of our knowledge respecting these remarkable phenomena, videlicet, the *casual magnetic disturbances*, or *magnetic storms* Antecedently to the formation of the German Magnetic Association and the publication of its first Annual Report in 1837, our information concerning them went no further than that there occurred at times, apparently not of regular recurrence, extraordinary agitations or perturbations of the magnetic needle, which had been noticed in several instances to have taken place *contemporaneously* in parts of the European continent distant from each other, and to have been accompanied by remarkable displays of Aurora, seen either at the locality itself where the needle was disturbed, or observed contemporaneously elsewhere The opinion which appears to have generally prevailed at this time, was that the Aurora and the magnetic disturbances were kindred phenomena, originating probably in atmospherical derangements, or connected at least in some way with disturbances of the atmospherical equilibrium. They were classed accordingly as "Meteorological Phenomena," and were supposed to have a local, though it might be in some instances a wide, extension and prevalence

The special purpose of the German Magnetic Association was to subject the "*irregular magnetic disturbances*" (as they were then called in contradistinction to the regular periodical and secular variations) to a more close examination, by means of systematized observations made simultaneously in many parts of Germany. With this view, six concerted days in each year were set apart in which the direction of the declination-magnet should be observed with great accuracy, by methods then for the first time introduced, at successive intervals of five minutes for twenty-four consecutive hours, the meteorological instruments being observed at the same time The clocks at all the stations were set to Gottingen mean time (Gottingen being the birth-place of the Association), and the observations were thus rendered strictly simultaneous throughout The high respect *entertained for the eminent persons with whom the scheme of the* Association originated, obtained for it a very extensive cooperation, not limited to Germany alone, but extending over a great part of the European Continent. The observations of the "Term-days," as they were called, were maintained until 1841, and were all transmitted to Gottingen for coordination and comparison

The principal results of this great and admirably conducted cooperative undertaking were published in works well known to magneticians. They may be summed up as follows —The phenomena which were the subjects of investigation were shown to be of casual and not regular occurrence, to prevail contemporaneously everywhere within the limits comprehended by the observations, and to exhibit a correspondence surprisingly great, not only in the larger, but even in almost all the smaller oscillations, so that, in the words of the Reporters, MM Gauss and Weber, "nothing in fact remained which could justly be ascribed to *local* causes."

Equally decided were the conclusions drawn against the previously imagined connexion between the magnetic disturbances and derangements of the atmosphere, or particular states of the weather. No perceptible influence whatsoever on the needle appeared to be produced either by wind-storms or by thunder-storms, even when close at hand.

The correspondence in the simultaneous movements of the declination-magnet, so strikingly manifested over an area of such wide extent, was however more remarkable in respect to the *direction* of a perturbation than to its *amount* The disturbances at different stations, and even, as was expressly stated, at *all* the stations, coincided, even in the smaller instances, in time and in direction, but *with dissimilar proportions of magnitude* Thus it was found generally that by far the greater number of the anomalous indications were smaller at the southern stations and larger at the northern, the difference being greater than would be due to the difference in the antagonistic retaining force (i e. the horizontal force of the earth's magnetism, which is greater at the southern than at the northern stations). The generality of this occurrence led to the unavoidable inference, that, in Europe, the energy of the disturbing force must be regarded weaker as we follow its action towards the south

A close and minute comparison of the simultaneous movements at stations in near proximity to each other led to the further conclusion, also stated to be unavoidable, that "various forces must be admitted to be contemporaneously in action, being probably quite independent of each other, and having very different sources, the effects of these various forces being intermixed in very dissimilar proportions at various places of observation according to the directions and distances of these from the sources whence the perturbations proceed" (Resultate aus den Beob des Mag. Vereins, 1836. pp 99, 100.) The difficulty of disentangling the complications which thus occur at every individual station was fully foreseen and recognized, and the Report, which bears the initial of M. Gauss, concludes with the remark that "it will be a triumph of science, if at some future time we should succeed in reducing into order the manifold intricacies of the combinations, in separating from each other the several forces of which they are the compound results, and in assigning the source and measure of each"

Such was the state of the inquiry when it was entered upon by the Royal Society. The Report of the Committee of Physics drawn up (*inter alia*) for the guidance of the Magnetic Observatories established by H M. Government for a limited period in four of the British Colonies, bears date in 1840. The objects proposed by this Report were a very considerable enlargement upon those of the German Association, as well as an extension of the research to more distant parts of the globe. The German observations had been limited for the most part to one only of the three elements required in a complete investigation. When the German Association commenced its operations, the Declination was the sole element for which an apparatus had been devised capable of recording its variations with the necessary precision To meet the deficiency in respect to the horizontal component of the magnetic force, M Gauss constructed in 1837 his bifilar magnetometer, which was employed at Gottingen and at some few of the German stations, concurrently with the Declinometer, in the term observations of the concluding years of the Association But an apparatus for the corresponding observation of the vertical portion of the Force was as yet wholly wanting, without such an apparatus as a companion to the bifilar, no determination could be made of the perturbations or momentary changes of the magnetic Dip and Force and without a knowledge of these no satisfactory conclusion in regard to the real nature, amount and direction of the perturbing forces could be expected. The ingenuity of Dr Lloyd supplied the desideratum by devising the vertical force magnetometer, which, with adequate care, has been found scarcely, if at all, inferior to the bifilar in the performance of its work. The scheme of the British Observatories was thus enabled to comprehend all the data required for the investigation of the casual disturbances, whether that investigation was to be pursued as before by concerted simultaneous observations at different stations, or, as suggested in the Report, *by the determination of the laws, relations and dependencies of the disturbances at individual stations obtained independently and without concert with other observers or other stations.* Thus, in reference to these particular phenomena, the British system was both an enlargement and an extension of the objects of the German Association, but it also embraced within its scope the determinations with a precision, not previously attempted, of the *absolute values* of the three elements, and of *the periodical and progressive changes* to which they are subject, premising however, and insisting with a sagacity which has been fully justified by subsequent experience, on the necessity of eliminating in the first instance the effects of the casual and transitory variations, as an indispensable preliminary to a correct knowledge and analysis of the progressive and periodical changes. A further prominency was given to investigations into the particular class of phenomena which form the subject of this paper, by the declaration that "the theory of the transitory changes is in itself one of the most interesting and important points to which the attention of magnetic inquirers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a much more perfect knowledge of these causes than we now possess"

The instructions contained in the Royal Society's Report for the adjustments and manipulation of the several instruments provided for these purposes were clear, simple and precise In looking back upon them after the completion of the services for which they were designed, *it is impossible to speak of the instructions otherwise than* with unqualified praise But the guidance afforded by the instructions terminated with the completion of the observations To have attempted to prescribe the methods by which conclusions, the nature of which could not be anticipated, should be sought out from observations not yet made, would have been obviously premature Yet without some discussion of the results, the mere publication of unreduced observations is comparatively valueless It has been well remarked by an eminent authority, whose opinions expressed in the Royal Society's Report have been frequently referred to in the course of this paper, that "a man may as well keep a register of his dreams, as of the weather, or *any other set of daily phenomena,* if the spirit of grouping, combining, and eliciting results be absent" It was indispensable that the attempt should be made to gather in at least the first fruits of an undertaking on which a considerable amount of public money and of individual labour had been expended, and the duty of making the attempt might naturally be considered to rest on the person who had been entrusted with the superintendence of the Government Observatories. The methods and processes adopted for reducing, combining, eliminating, and otherwise eliciting results were necessarily of a novel description, they were in fact an endeavour to find a way by untrodden paths to simple and general phenomenal laws where no definite knowledge of the origin or mode of causation of the phenomena previously existed Happily it is not necessary to trespass on the time or attention of the Society by a description of the methods and processes which have been employed to elucidate some of the leading features of the magnetic storms, as these are fully described in the discussions prefixed to the ten large volumes in which the observations at the Colonial Observatories have been printed It will be only necessary to advert, and that very briefly, to some of the principal conclusions which may be supposed to throw most light on the theory of these phenomena.

The results of the extension of the term-day comparisons to the

American Continent, and to the Southern Hemisphere and the Tropics, may first be disposed of in a very few words. The contemporaneous character of the disturbances, which had been shown by the German term-observations to extend over the larger portion of the European Continent, manifested itself also in the comparisons of the term-days in 1840, 1841, and 1842 at Prague and Breslau in Europe, and Toronto and Philadelphia in America, published in 1845, and the same conclusion was obtained by comparing with each other the term-days at the Colonial Observatories, situated in parts of the globe most distant from one another The days of disturbance still appeared to be of casual occurrence, but were now recognized as affections common to the *whole globe*, showing themselves simultaneously at stations most widely removed from each other When distant stations were compared, as for example stations in Europe with those in America, and either or both with Tasmania, discrepancies in the amount of particular perturbations, similar to those which had been found in comparing the European stations with each other, presented themselves, but larger and more frequent, and extending occasionally even to the *reversal* of the *direction* of the simultaneous disturbance Instances were not unfrequent of the same element, or of different elements, being disturbed at the same observation-instant in Europe and America, and on the other hand, there were perturbations, sometimes of considerable magnitude, on the one continent, of which no trace was visible on the other. Hence it was concluded, with the increased confidence due to this additional and more extensive experience, that various forces proceeding from different sources were contemporaneously in action, and it was further inferred that the most suitable and promising mode of pursuing the investigation was by an endeavour to analyse the effects produced at individual stations, and to resolve them if possible into their respective constituents.

The hourly observations which had been commenced at the Colonial stations in 1841 and 1842, and continued through several subsequent years, furnished suitable materials for this investigation, the first fruits of which were the discovery, that the disturbances, though casual in the times of their occurrence, and most irregular when individual perturbations only were regarded, were, in their *mean* effects, *strictly periodical phenomena*, conforming in each element, and at each station, on a mean of many days, to a law dependent on the solar hour, thus constituting a systematic mean diurnal variation distinct from the regular daily solar-diurnal variation, and admitting of being separated from it by proper processes of reduction This conformity of the disturbances to a law depending on the solar hours was the first known circumstance which pointed to the sun as their primary cause, whilst at the same time a difference in the *mode* of causation of the regular- and of the disturbance-diurnal variations seemed to be indicated by the fact, that in the disturbance-variation the local hours of maximum and minimum were found to vary (apparently without limit) in different meridians, in contrast to the general uniformity of those hours in the previously and more generally recognized regular solar-diurnal variation

This first reference of the magnetic storms to the sun as their primary cause, was soon followed by a far more striking presumptive evidence of the same, by a further discovery of the existence of a periodical variation in the frequency of occurrence, and amount of aggregate effects, of the magnetic storms, corresponding in period, and coincident in epochs of maximum and minimum, with the decennial variation in the frequency and amount of the spots on the sun's disk, derived by Schwabe from his own systematic observations commenced in 1826 and continued thenceforward The decennial variation of the magnetic storms is based on the observations of the four widely distributed Colonial Observatories, and is concurred in by all This remarkable correspondence between the magnetic storms and physical changes in the sun's photosphere, of such enormous magnitude as to be visible from the earth even by the unassisted eye, must be held to terminate altogether any hypothesis which would assign to the cause of the magnetic disturbances a local origin on the surface or in the atmosphere of our globe, or even in the terrestrial magnetism itself, and to refer them, as cosmical phenomena, to direct solar influence, leaving for future solution the question of the *mode* in which that influence produces the effects which we believe we have thus traced to their source in the central body of our system*.

* The existence of a decennial period of the magnetic storms was not, as some have supposed, a fortuitous discovery; but a consequence of a process of examination early adopted and expressly devised, by the employment of a *constant* separating value, to make known any period of longer or shorter duration which might

We may regard as a step towards this solution the separation of the disturbances of the declination into two distinct forces acting in different directions and proceeding apparently from different foci, the phenomena of distinct (though in so many respects closely allied) variations exhibit the same peculiar features at all the stations to which the analysis has hitherto extended, and have been exemplified by the observations at Kew, as shown in the early part of this paper. A similar separation into two independent affections, each having its own distinct phenomenal laws, has followed from an analysis of the same description applied to the disturbances of the magnetic dip and force at the Colonial stations, thus placing in evidence, and tracing the approximate laws of the effects of six distinct forces (two in each element) contemporaneously in action in all parts of the globe, and pointing in no doubtful manner to the existence of two terrestrial foci or sources in each hemisphere from which the action of the forces emanating from the sun and communicated to the earth may be conceived to proceed. Such an ascription naturally suggests to those conversant with the facts of terrestrial magnetism the possibility that Halley's two terrestrial magnetic foci in each hemisphere may be either themselves the localities in question, or may be in some way intimately connected with them. The important observations which we owe to the zeal and devotion of Captain Maguire, R. N. and the Officers of H. M. S. 'Plover,' have made us acquainted with Point Barrow as a locality where the magnetic disturbances prevail with an energy far beyond ordinary experience, indicating the proximity of that station to the source or sources from which the action of the forces may proceed. Now Point Barrow is situated in a nearly intermediate position between what we believe to be the present localities of

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fall within the limits comprised by the observations. The period being decennial, and the epoch of minimum occurring at the end of *1843 or beginning of 1844*, the epoch of maximum was necessarily waited for in order to ascertain the precise duration of the cycle. The maximum took place in 1848–1849, the observations in 1850 and 1851 showing that the aggregate value of the annual disturbances was again diminishing as it had been in 1842 and 1843. The process of determining the proportion of disturbance in different years is a somewhat laborious one, and requires time but in March 1852, I was able to announce to the Royal Society *the existence of a decennial variation*, based on the concurrent testimony of the observations at Toronto and Hobarton, deeming it proper that so remarkable a fact should not be publicly stated until it had been thoroughly assured by independent observations at two very distant parts of the globe.

Halley's northern foci, and at no great distance from either: in such a situation the exposure to disturbing influences proceeding from both might well be supposed to be very great. The displays of Aurora at Point Barrow exceed also in numerical frequency any record received from any other part of the globe.

The further prosecution of this investigation appears to stand in need of some more systematic proceeding than would be supplied by the uncombined efforts of individual zeal Observations similar to those of the Kew Observatory, made at a few stations in the middle latitudes of the hemisphere, distributed with some approach to symmetry in their longitudinal distances apart, would probably furnish data, which by their combination might serve to assign the localities from whence the disturbances are propagated—contribute still further to disentangle the complications of the forces which produce them,—and thus hasten the attainment of that "triumph of science" foreseen and foreshadowed by the great geometrician of the last age Of such a nature was the *scheme contemplated by* the Joint Committee of the Royal Society and British Association, and submitted to H.M Government in the hope of obtaining their aid in the execution of such part of it as fell within British dominion, and of thus "maintaining and perpetuating our national claim to the furtherance and perfecting of this magnificent department of physical inquiry" (Herschel in 'Quarterly Review,' September 1840, p. 277) The scheme was no unreasonable one: probably eight or nine stations in the contour of the hemisphere might suffice; and of these we already possess the observations at Toronto; those at Kew are in progress; and self-recording instruments, similar to those at Kew, are now under verification at Kew preparatory to being employed on the Western or Pacific side of the United States Territory, at a point not far from the previously desired Station of Vancouver Island, for which a substitute is thus provided. This Observatory, as well as one at Key West on the southern coast of the United States, in which self-recording instruments are already at work, will be maintained under the authority and at the expense of the American Government, and both have been placed under the superintendence of the able and indefatigable director of the "Coast Survey," Dr Alexander Dallas Bache. The Russian Observatory at Pekin, the trustworthy observations of which are already known to the Society, is understood to have recommenced its hourly observations, and stands only in need of an apparatus for the vertical force (which might be readily supplied from this country), to contribute its full complement to the required data. More than half the stations may therefore be regarded as already provided for, and there are other Russian observatories in the desired latitudes and longitudes which might be completed with instruments for a full participation

It would be wrong to conclude these imperfect notices withont recognizing how greatly the researches have been aided in their progress by the united and unfailing countenance and support of the Royal Society and of the British Association. The Kew Observatory owes its existence and maintenance to funds most liberally supplied from year to year by the British Association, and the cost of the self-recording magnetic instruments, of which the first instalment of the results has formed the early part of this paper, was supplied from funds at the disposal of the Council of the Royal Society. Magnetical science, rapidly as it is advancing, is even yet in its infancy, and it is in their early stages particularly that all branches of natural knowledge stand in need of the fostering aid of societies in which science is valued and cultivated for its own sake

November 22, 1860

Major-General SABINE, R.A., Treasurer and Vice-President, in the Chair.

John Thomas Quekett, Esq, was admitted into the Society

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting, and the list of Council and Officers proposed for election was read as follows —

President —Sir Benjamin Collins Brodie, Bart, D C.L.

Treasurer.—Major-General Edward Sabine, R A, D C L.

Secretaries.— William Sharpey, M D, LL D.
George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—William Hallows Miller, Esq, M.A.

Other Members of the Council —John Couch Adams, Esq., Sir

John Peter Boileau, Bart.; Arthur Cayley, Esq., William Fairbairn, LL.D; Hugh Falconer, M.D, William Farr, M.D., D C.L., Thomas Graham, Esq, M.A, D C L, Sir H Holland, Bart, M D, D.C.L., Thomas Henry Huxley, Esq; Sir J G Shaw Lefevre, M.A, D.C.L, James Paget, Esq, Joseph Prestwich, Esq, William Spottiswoode, Esq., M A., John Tyndall, Ph.D., Alex William Williamson, Ph D, Col. Philip Yorke

The following communications were read.—

I "On Boric Ethide." By EDWARD FRANKLAND, Ph.D, F.R S, and B. DUPPA, Esq. (See p. 568.)

II. "On Cyanide of Ethylene and Succinic Acid"—Preliminary Notice By MAXWELL SIMPSON, Ph.D. (See p 574.)

III. "Results of Researches on the Electric Function of the Torpedo" By Professor CARLO MATTEUCCI of Pisa. In a Letter to Dr. SHARPEY, Sec. R.S. (See p. 576)

IV. "Natural History of the Purple of the Ancients" By M LACAZE DUTHIERS, Professor of Zoology in the Faculty of Sciences of Lille. (See p. 579)

V. "Contributions towards the History of Azobenzol and Benzidine." By P. W. HOFMANN, Ph.D (See p 585.)

VI. "On Bromphenylamine and Chlorphenylamine." By E. T MILLS, Esq (See p 589.)

VII "New Compounds produced by the substitution of Nitrogen for Hydrogen." By P. GRIESS, Esq. (See p. 591)

VIII. "Contributions towards the History of the Monamines."—No III. Compound Ammonias by Inverse Substitution. By A. W. HOFMANN, LL D., F.R.S &c. (See p. 594.)

IX. "Notes of Researches on the Poly-Ammonias."—No. IX. Remarks on *anomalous* Vapour-densities. By A. W. HOFMANN, LL.D., F R.S. &c. (See p. 596.)

X. "Notes of Researches on the Poly-Ammonias."—No. X.

On Sulphamidobenzamine, a new base, and some Remarks upon Ureas and so-called Ureas By A. W. Hofmann, LL.D., F R.S. &c. (See p 598)

XI. "Researches on the Phosphorus-Bases."—No. VIII. Oxide of Triethylphosphine By A W. Hofmann, LL.D., F.R.S. &c. (See p 603.)

XII "Researches on the Phosphorus-Bases"—No IX. Phospharsonium Compounds. By A. W Hofmann, LL.D., F.R S &c (See p 608.)

XIII. "Researches on the Phosphorus-Bases"—No. X. Metamorphoses of Bromide of Bromethylated Triethylphosphonium. By A W Hofmann, LL D., F.R S &c. (See p 610.)

XIV "Researches on the Phosphorus-Bases"—No. XI. Experiments in the Methyl- and in the Methylene-Series. By A. W Hofmann, LL.D., F.R.S &c (See p 613.)

XV. "Researches on the Phosphorus-Bases."—No. XII. Relations between the Monoatomic and the Polyatomic Bases By A. W. Hofmann, LL D, F R.S. &c. (See p. 619.)

XVI "On the Physiological Anatomy of the Lungs." By James Newton Heale, M D. Communicated by Sir B. C Brodie, Bart., P R.S Received August 28, 1860

(Abstract)

The arrangement observed in the divisions and subdivisions of the bronchial tube is that of a panicle. There is everywhere to be distinguished a straight diminishing tube, from which lesser tubes are given off alternately from its sides, these lesser tubes in their turn observe a similar plan of distribution, and the smaller tubes, down to their ultimate terminations, are governed by the same system. There is nowhere to be found either a true dichotomous or a trichotomous division.

The distinction between that part which is bronchial tube and that which is parenchyma, is, in a properly injected lung, marked and very decisive, and can never be mistaken by even the most inexperienced person, when a fragment, however small, is examined by the microscope.

When the bronchial tubes have reached their penultimate terminations, the coats which form their perimeters split, as it were, into two layers The outer of these, which is tougher, thicker, and more fibrous, expands and encloses an ultimate portion of the parenchyma To these portions of the lung the name of 'leaflets' is given in this treatise. The outer coat of the bronchial tube, by being spread out in the leaflets, becomes continuous with the general parenchyma of the lungs. The inner portion of the tube immediately divides into numerous minute tubes, to which the name of 'pedicels' is now given.

Each of the pedicels goes to a different leaflet, but each leaflet receives several pedicels from different terminal bronchial tubes

A minute anastomosis is thereby established between the terminations of the different bronchial tubes, through the leaflets. On entering the leaflet, each pedicel splits up into processes which extend to the internal perimeter of the leaflet, and by intersections with similar processes derived from the other pedicels, divide the interior into compartments called 'air-cells'

Each leaflet is, to a considerable extent, divided from those which surround it by sulci, but it is continuous in structure with them by certain parts of its base, in which it is in contact with them and adherent.

On the surface of the lungs these leaflets give the appearance of bodies of a somewhat quadrilateral shape.

The interior of all the bronchial tubes is marked with 'rugæ,' and these rugæ show the direction of the bundles of longitudinal contractile fibres, which are placed immediately beneath the mucous membrane. These longitudinal fibres are surrounded by circular ones, and it is by the contraction of these latter that the rugæ are formed When the bronchial tubes have been kept distended, the rugæ are not present, the mucous membrane is then perfectly smooth.

There are no such things as 'alveoli' belonging to the tube.

The bronchial artery supplies the following structures:—

I. The successive layers of cellular tissue, the lymphatic glands, coats of the pulmonary vessels, neurilemma, &c

II The fibro-cartilaginous and fibrous portion of the bronchial tubes, some exceedingly minute capillaries, derived from these, extend into the mucous membrane, but do not in any way anastomose with the proper vascular plexus belonging to this structure

III The bronchial artery also freely supplies the walls and processes of the leaflets with arterial blood.

IV Some small branches arrive at the surface of the lungs, being conducted thither by some minute bronchial tubes, which communicate with longitudinal air-passages to be found in the substance of the pleura These small arteries anastomose freely with other branches of the same artery in the sub-pleural cellular tissue.

The bronchial artery forms no sort of anastomosis with the pulmonary system in any part of the lungs, and is quite incapable of discharging the function of the latter under any circumstances whatever

The bronchial *veins* are of two sorts one forms a very free system of inosculation on the surface of the lungs, the other is always discoverable (in the recent lung) in the loose cellular tissue surrounding the bronchial tubes. they both have *valves*, and consequently cannot be injected in a retrograde direction, but they can both be injected from the bronchial artery. Neither of them can be injected under any circumstances from the pulmonary system They both have large intercommunicating trunks

The pulmonary artery accompanies the bronchial tube, dividing precisely as it divides, wherever there is a bronchial tube, however small, there is likewise a corresponding pulmonary artery, and never more than one It gives off no branches to any collateral structure, and it forms no inosculations either with its own branches or with any other vessels, every portion of it ultimately reaches the leaflets, and there it makes a most minute, uniform and equal reticulation, anastomosing throughout the lung

The pulmonary *vein* commences by *tufts* in the interior of the leaflets, part of the capillary vessels emerges on to the surface of the leaflet, and commences the formation of minute veins, which immediately dip down through the sulci which divide the leaflets, to reach the interlobular surfaces, where they increase in size, and ultimately come into contact with the under surface of a bronchial tube · the other part of the capillary vessels makes its way from the interior of the leaflet by means of the pedicel, and reaches the mucous membrane, where a most abundant, minute, and exceedingly regular plexus is formed, occupying the whole surface of the mucous membrane.

This plexus in the larger tubes is reinforced by blood-vessels derived from numerous leaflets which surround the bronchial tubes, straight vessels penetrate from these leaflets through the walls of the bronchial tubes, to reach the plexus in the mucous membrane

On the external surface of the bronchial tubes, numerous radiating vessels collect the blood from the plexus in the mucous membrane, and the trunks of these radiating vessels soon terminate in the veins, already described as coming into contact with the under surface of the bronchial tube.

The vessels which have been alluded to as receiving their tributaries in the interlobular surfaces, collect their blood from all the surrounding lobules, consequently the blood which reaches the vein placed in contact with a particular bronchial tube, is not derived exclusively from the same lobules as those with which that bronchial tube and its accompanying artery are in connexion, but it receives its blood from all parts of the lungs promiscuously.

The pulmonary veins accompanying the bronchial tubes continue to increase in size in proportion as the tubes themselves increase, and finally they terminate in the large veins which enter the left auricle.

XVII. "On the Curvature of the Indian Arc" By the Venerable J. H. Pratt, Archdeacon of Calcutta. Communicated by Prof Stokes, Sec R.S. Received Sept. 3, 1860

(Abstract)

This communication completes the series of the author's papers on the subject of the Indian Arc He commences by recapitulating the chief results of his former calculations, and adverting to the attempt which he made in his former papers to explain the difficulty which those calculations brought to light, namely, that the amplitudes of the arcs from Kalıana to Kalıanpur and from Kalıanpur to Damargida, determined geodetically, were so little in excess as they proved to be of the same amplitudes determined astronomically,—a difficulty which he endeavoured to get over by attributing to the Indian Arc a curvature different from that corresponding to the mean meridian of the earth. In the present communication, introducing the condition that the length of the chord of the arc must be the same in both the ellipses, the local and the mean, drawn through the

stations at the extremities of the arc, he demonstrates that no change in the curvature of the arc, within reasonable and indeed within wide limits, can have any appreciable effect on the calculated amplitude The author's conclusions from the whole investigation regarding the Indian Arc are thus summed up —

(1) Colonel Everest discovered that the astronomical amplitudes of the two portions of the Indian Arc between Kaliana and Kalianpur, and between Kalianpur and Damargida, are, the first less by 5″ 24, and the second greater by 3″ 79, than the geodetic amplitude calculated with the mean semi-axes and ellipticity of the earth

(2) The geodetic amplitudes of these two portions of the arc, calculated from the measured lengths and with the mean axes, will be sensibly exact, even should the curvature of the arc differ from that of the mean meridian within reasonable but wide limits—a thing which geology teaches us to be very likely the case

(3) Hence the geodetic measurements of the survey being without sensible error, as is known by the tests applied, the discrepancy in (1) can only arise from local attraction affecting the vertical line, and so changing the astronomical amplitudes

(4) Two great visible causes of disturbance of the vertical by attraction are, the Mountain Mass on the north of India, and the Ocean on the south. The influence of both of these is felt all over India, the first producing a northerly deflection, varying from 27″ 98 at Kaliana to a sensible angle (probably about 3″, but this the author has not calculated) at Cape Comorin; the second producing also a northerly deflection, varying from about 19″·71 at Cape Comorin to 6″·18 at Kaliana

(5) The combined effect of these two visible causes is to make the astronomical amplitude of the upper arc 13″ 11 too small, and of the lower 3″ 82 also too small They are therefore insufficient to account for the discrepancies pointed out by Colonel Everest. Some other cause must exist tending to increase the upper astronomical amtude by 13″·11−5″ 24=7″ 87, and also to increase the lower amplitude by 3″ 82+3″ 79=7″ 61

(6) It has been demonstrated that a slight but wide-spread variation in the density of the crust, from that deduced from the fluid-theory, either in excess or defect, such as there is no difficulty in conceiving to exist, is sufficient to account for deflections such as

these. For example · an excess of density amounting only to $\frac{1}{50}$th part, extending through a circuit of about 120 miles around the mid-point of the whole arc between Kalıana and Damargida (and therefore not far from Kalianpur), and to a depth of about 200 miles, will produce this effect, and make the calculated deflections from the three causes—the mountains, the ocean, and this hidden cause below —exactly accord with the observed errors in the astronomical amplitudes.

(7) The resulting total deflections at Kaliana, Kalianpur, and Damargida, arising from the three causes, are 26″ 29, 21″ 05, and 24″ 84 these make the two astronomical amplitudes, the one 5″ 24 smaller, and the other 3″ 79 larger than the geodetic amplitudes, the error brought to light by Colonel Everest

(8) No error can arise in the mapping of India by using the geodetic measurements But the position of any places put into the map thus formed, the latitudes of which are determined from observations of the sun or any other heavenly body, will be wrong by the total amount of the deflection at those places, amounting in the three cases above to as much as half a mile.

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